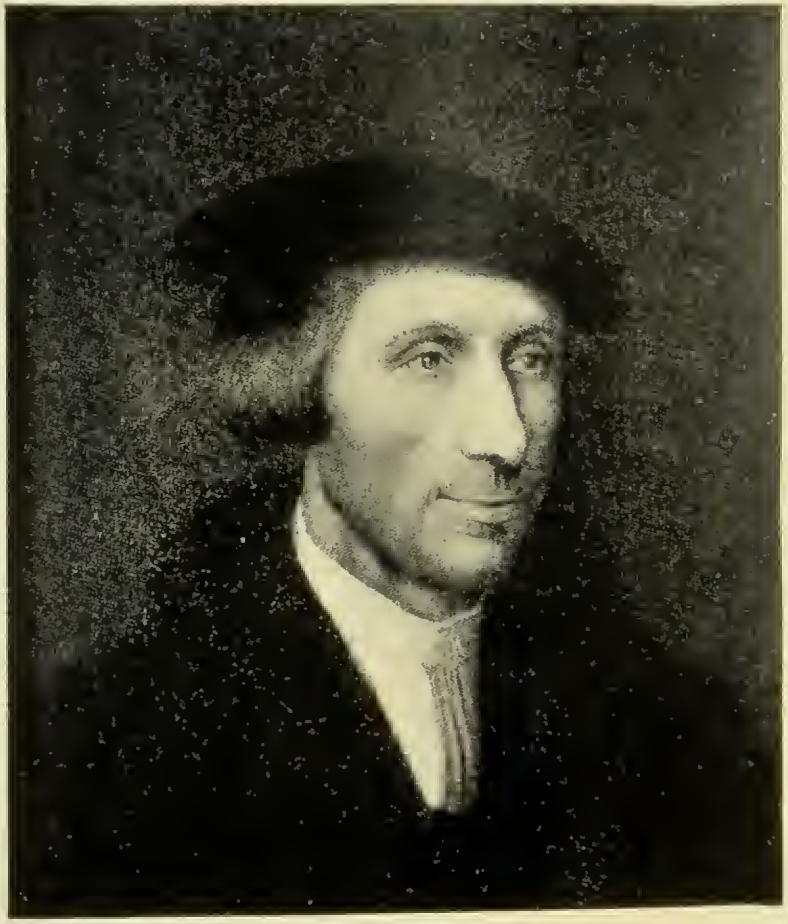
Edward A. Flanery

TWELVE CATHOLIC MEN
OF SCIENCE



THOMAS LINACRE

TWELVE CATHOLIC MEN OF SCIENCE

EDITED BY

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PREFACE

The object of these biographies is to demonstrate the fact, unknown apparently to many critics of the Church, that there are numerous stars of science (and many more than this series includes) who were also devout Catholics, and found no difficulty in maintaining both positions simultaneously. It ought to be superfluous to maintain such a thesis, and it would be so if persons who propagate such accusations as are made against the Catholic Church would, first of all, investigate the facts of the case. But, as the statement has been made, it may be well to give some definite examples of the co-existence in the same individual of scientific enthusiasm and reputation with a steadfast attachment to the doctrines of the Catholic Church.

As only a selection is possible, it has been thought desirable that such selection should embrace a collection of subjects as varied as possible. Hence the persons whose lives are here sketched are taken from different countries, from different ages, and from different branches of science—biological and physical. Some—*e.g.* Stensen, Secchi, and Mendel—were churchmen, others were lay; some spent their lives amongst Catholic surroundings, others, like Dwight, lived largely

in a non-Catholic environment. Most of them were born in the Church, but some entered in later years—*e.g.* Stensen and Dwight. Some lived in times when the profession of their religion was at least no disadvantage, others—*e.g.* Laennec and Pasteur—when it was either a positive disadvantage or, at least, in no way in their favour. In one thing only all are alike, and that is in their attachment to their religion.

BERTRAM C. A. WINDLE.

UNIVERSITY COLLEGE, CORK,
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THOMAS LINACRE

SCHOLAR, PHYSICIAN, PRIEST

(1460–1524)

BY

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THE career open to the talents is supposed to date from Napoleon's time, though not everybody is satisfied that the way is open even now. "Educational ladders" up which may be had, if not a particularly easy way, still a way from any lower level to the highest place in the State, are talked of. The elevating apparatus (it is held) must include some arrangement for study abroad in the shape of travelling subsidies, with a view to mental expansion and uplifting under the influence of foreign life and manners.

If that is the best that education can do (and Mr. Ruskin held that Englishmen are constitutionally unfit to do it), the people of the fifteenth century must be allowed to have had some notion of what was good for them. At least, they and their times did well for the boy Thomas Linacre (Lynacre), born at Canterbury 1460 (*tempore* Henry VII.); educated at the monastery school—parents obscure or unknown; thence to Oxford, Florence, Rome, Padua; the first, or amongst the first, of Renaissance scholars; friend of Erasmus and More and many others, great men in a great time—Wolsey, Warham, Colet; held in high repute amongst Italian litterati and princes—Lorenzo de' Medici and his son Pope Leo X.

All will recognize the truth of the statement that, amongst the early Humanists, scholarship claimed a precedence that has not been equalled since that day.

There are two remarks to be made before approaching the facts of Linacre's life. For one thing, nobody denies that the Church, since its foundation to the present hour, has recognized that the calling to its ministry is independent of rank or position. But Linacre, though he took priest's orders, did so late in life. He was over fifty years of age and already a famous man when ordained.

Again, the policy of Henry VII. made preferment by merit alone a comparatively common thing. The King had much to do with the making of England. It was to his interest to depress the power of the nobles—even those by whose help he had risen; the caprice of individual leaders had often determined unexpected victory in the Wars of the Roses. It was the policy of Richelieu and of Charles V. Men of humble origin were sought out and appointed chiefs of State, the efficiency thereby secured strengthening the power of the Crown. The process also strengthened the State, though, possibly, the intention here was not so direct.

Last of all, and most worthy of being said, is this—that the new class raised to power were conspicuous by their liberality towards education. Linacre left his great wealth—almost all of it—for public use. The gifts were made during his lifetime. There was little left to dispose of in his will (proved in 1525). We learn from small bequests that he had a brother and sisters—almost all we know of his family. These foundations of the fifteenth and early sixteenth century endure—those of them that passed the danger period of Henry VIII.'s Commissions; and their endurance is evidence of the good intentions and sound judgement of the benefactors. Characteristic of them, if not their dominant character, is the personal relation set up between giver and recipient. The existence of a soul was emphasized—on that ground, at least,

both parties stood equal—"Of what he had he could give," and the prayer for the benefactor went far to balance the account between the two parties.

To the same influence is due the democratic policy that pervades the old foundations. All men were equal for the purposes of these charitable trusts.

Linacre was a product of the early Italian Renaissance: there was free intercourse between Italy and England in his time. William de Selling had introduced Humanistic learning, in its new and attractive form; and at the monastery school of Christ Church, Canterbury, where Selling taught (he was afterwards Prior), Linacre came under his influence. The impress was never lost. At Oxford (probably All Souls College, where Linacre was elected Fellow in 1484) he came under the influence of Vitelli, said to have been the first teacher of Greek in England; at Oxford, too, were Grocyn and Latimer. The "atmosphere" could not be more "humanistic," and the fervour that attends nascent culture more than made up for the formalities of organized procedure. I am often set wondering by the mass of regulations and statutes modern universities think fit to set up when starting house on their own account, as if rules could take the place of men! The "mortmain" of paper constitutions is not a good substitute for living personal influence; worse than that, the paper formulæ tend to oust the influence of the living agents. I like to think of Oxford at the close of the fifteenth century: then, as now, a place to dream in, but where, as not now, the student had the full enjoyment of the founders' intentions. The college chapels, that to-day can only suggest the purpose of the early benefactors, were then its living embodiment.

In 1485 Selling went to Rome as ambassador from Henry VII. to the Pope, and Linacre travelled in his suite. The master did not forget his old pupil, and

Linacre's popularity abroad, while due, in the main, to personal qualities, must have owed something to Selling's introductions. The universities at that time formed a great guild of persons animated with a common purpose; no student was a stranger at any of them. The "nations" of mediæval universities were a recognition of the cosmopolitan character of their influence; yet with the cosmopolitan idea there was found a unifying influence derived from a central authority. "In spite of national diversities there existed all over Europe a striking unity of spirit, of civilization, of learning and religious feeling, diffused mainly by the Church, which from her centre at Rome acted as the mainspring of mental cultivation everywhere, and penetrated into the internal constitution of all the nations beneath her sway." This, says the learned historian of the *English Universities*, Professor Huber of Marburg, must be our guiding light in studying the origin and growth of universities. He goes on to say that long before the period of revived classical learning, the Church manifested an intellectual spirit and an objective historical method in her mode of treating the Holy Scriptures, the Fathers of the Church, the ancient writers and their languages, the discoveries made by that age in natural philosophy; and that the schools and universities were organized and extended in a manner parallel to the progress of the intellectual life of the time.

One might expect criticism from Marburg not to be too friendly to Catholic claims, but Huber's judgement as a historian is too just to allow him to endorse the prejudiced view that the Catholic Church has not favoured the progress of learning. Most of the continental universities, he admits, originated in entire dependence on the Church, and her exercise of so important a trust was marked by an honourable activity. In our own country the old English universities are, as well as we can trace, offshoots of Paris, while St. Andrews, Aberdeen, and Glasgow owe their foundation directly to

Papal Bulls. In Ireland a Papal foundation of a university was granted long before Trinity College was thought of.[1]

Linacre visited Bologna, the oldest university; then Florence, where Lorenzo de' Medici patronized the young Englishman, allowing him to share the studies of Piero and Giovanni de' Medici (the latter afterward Pope Leo X., and not unmindful of his old fellow-student). A year later came Rome and the Vatican manuscripts, and Linacre's reputation as an authority in Humanistic learning was established.

It must be remembered in these days of cheap editions that the early Humanists worked from manuscripts. Scholarship and judgement in a high degree were needed to prepare publications for the press. There were not many publishing establishments—among them the Aldine Press at Venice stood out conspicuously. Aldus himself quotes "Thomas Anglicus" (Linacre) as a skilled witness to the accuracy of the work done at Venice. Those who till lately accepted the *litteræ humaniores* as the only fitting preparation for Church and State, or who hold that opinion still, must not forget the service rendered by the early Humanists. There was fine scholarship amongst them, so far as the text of an author and its artistic interpretation went; the day of scientific philology had not arrived. For them the paramount duty was, so they thought, to place before the world the buried treasures of antiquity, and to that end they spared no labour. The deciphering of manuscripts was no light work. Printer and scholar worked together; often, as at the famous Aldine Press, the printer was a scholar. But the first duty was to publish, to make the author known, and known in a text that people could depend on.

What led Linacre to specialize in the works of the old

[1] Clement V. (1312), John XXII. (1320). Sir Philip Sidney in 1568 attempted to restore the Papal university of Dublin.

medical writers is not known—possibly the practical bent of the English mind to bring out something useful. He studied medicine at Padua and Vicenza. Some of the teachers were, like himself, students of medicine and Humanistic scholars.

It was the custom (still surviving) to proceed to a degree by " disputation," and there is a record of the brilliant performance when Linacre defended his " thesis " against the senior professors at Padua. He obtained his degree of M.D., and soon after returned home.

Travelling was slower work then than now. One would like to have the impressions of the graduate of Oxford and Padua on Italy and England, and the life of the time, but Linacre has left us little as to this. We only know that he returned by Geneva, Paris, Calais; and of what befell him by the way, nothing except the charming story of his last look on Italy. This he had from the top of the mountain boundary, probably the Great St. Bernard, and there he raised a rough cairn of stones—an altar to his " sancta mater studiorum." We have since changed the term to " alma mater " when we speak of a university, as we have changed many other academic practices of the past.

What was Italy, then, to move her student to such feeling ? Long before, her own children had addressed her in similar language. Virgil's impassioned address to " the generous land, the great mother of heroes," was probably in Linacre's mind. But it would be a mistake to suppose that the Middle Ages, before Humanism became dominant, were unproductive. Scholasticism preceded Humanism, and, as to the literary influence of Scholasticism, one has but to point to its poet, Dante.

In Scholasticism organization was carried to extremes, but it must be judged by its time. When feudalism prevailed, when printing was unknown, when might was right, not alone in the great states, but in every little baronial jurisdiction, there was authority to which appeal

might still be made—the Catholic Church. Its agents kept up free communication between the centre at Rome and each episcopal territory. In theory, and in great measure in practice, when Rome spoke all must obey. What a free press does, or might do, to-day, Rome undertook to do in the Middle Ages. Its power shielded the weak; before its spiritual tribunal rank counted for nothing. That was the Church at its best, a surer ground of judging than the taking things at their worst. For that *rôle* rigid organization was necessary,—an elaborate canon law and an elaborate intellectual apologia. That is the justification of Scholasticism.

But the Church never failed to support learning as such—the history of even one religious order, that of the Benedictines, is enough to prove this; and in Linacre's time the young Renaissance learning received cordial approval and support. Mr Ruskin, indeed, thinks it had too much support; he dates the decay of painting in Italy from the substitution of Renaissance for Biblical inspiration.

It is difficult to say exactly what is understood by the term Humanities. To-day it means classics—the Greek and Latin classics. In Linacre's time it seems to have meant not alone classics, but the recognition of the importance of man as an individual, and a claim, that scholarship must be looked on as a high aim. So far the support of the Church was freely given. Nicholas V. and Leo X. gave the full weight of their authority to the movement which spread from Italy to England, Germany, and France. Vittorino da Feltre, in Italy, gave the impulse that dominates still the English public schools. His school at Mantua set out as things to be aimed at:—Memory, recitation of passages from classical authors, correct analysis; physical culture and exercises were received free. We might almost think we were reading the programme of an English High School.

But when Humanism developed, as it unfortunately did, into licence, and the masters of the New Learning became notorious for evil living, the support of the Church was withdrawn. The early or Catholic Humanists are to be distinguished from the later; whatever relation there may be between Humanism and the Reformation has to do with the later phase. As Mark Pattison says in his life of Erasmus, that great scholar turned back from the prospect opened out when Humanism tried to set up in place of Scholasticism—which, whatever may be said of its formalism and over-elaboration, was at least constructive and elaborated in defence of morality—a new code of irresponsibility which must inevitably drift, as it did, into pagan licence.

Before the New Learning took that attitude it received the warm support of the Church. Nicholas V., during his eight years' Papacy (1447–1455), practically established Humanism in Italy. Scholars were encouraged; he gave ten thousand *gulden* for a metrical translation of Homer. His own erudition was such that his friend Æneas Silvius (afterwards Pius II.) says of him, "What he did not know was outside the pale of human knowledge." The Vatican Library is the best monument of this Pope.

On his return to England Linacre found himself famous. He was appointed tutor to the young Prince Arthur, Henry VIII.'s eldest brother, and Italian teacher to the Princess Mary; but Court favour did not turn his mind from medicine. Oxford welcomed back her brilliant student. The Padua M.D. was admitted "ad eundem gradum," and so began Linacre's influence on English medicine, which is a controlling one to the present day.

He gave some medical lectures at Oxford, but no doubt his practical mind saw that London must be the centre of operations, if effective work was to be done in medical reform.

In 1509 Linacre was made King's Physician to Henry VIII.; the annual pay was £50, about £500 of our money. It is known that Erasmus, Archbishop Warham, Colet, and More consulted him. His success was assured, and the riches he amassed must have been considerable. To one purpose his mind was set, from 1509 till 1518—the establishment of the profession of medicine on a firm and lasting basis; and towards that purpose he gave freely. On the 23rd September 1518 Henry VIII.'s charter constituting the Royal College of Physicians of London was issued, with Linacre as first President. The names of Wolsey and Linacre are cited as supporters of the prayer that led to the issue of the charter.

No professional foundation, at home or abroad, stands higher to-day in public estimation than this College. Its Fellowship is recognized as evidence of culture, professional skill, and high character: one might say that by it the attributes of the founder are preserved. Linacre took the Italian model as his guide in drafting the constitution of the College, but much was done at his own initiative.

The effect of tradition—unconscious often, "an atmosphere"—is seldom altogether absent from the life of an old foundation. Of this Oxford is a good example. If the Repealing Act of Elizabeth were itself repealed to-morrow, a Catholic might enter one of the old college chapels and hear Mass without any feeling of surprise. The solemn function would seem a natural reversion to the founder's intention and in entire harmony with the spirit of the place. In Ireland we know well what virtue there is in "atmosphere." The rulers of Queen Elizabeth's Dublin University College have tried again and again to bring it into touch with the bulk of Irish people, and every time the attempt has been barred by the impassable barrier of tradition. People say an Act of Parliament can do anything, but there are places where the King's writ does not run.

Besides the College of Physicians foundation, to which much of Linacre's wealth was devoted (the King's charter gave no money), he set up classes in medicine and Greek at the old universities. The Oxford endowment, after many vicissitudes, has been revived by the University Commissioners by the establishment of the Linacre Chair of Comparative Anatomy.

Linacre has been described as a "medical Humanist" —a not unfair estimate of his claim to public recognition.

Medicine and literature were closer together in the fifteenth and sixteenth centuries than they are now. Literature meant scholarship, it is true, but more.

The matter of an author, not merely his style, was valued, and, far more than that, the search for information in Greek writers led irresistibly to a search for facts at first hand everywhere. No doubt the search was qualified by the conviction that facts by themselves are of little value, and that to be of much interest they must be subjected to the influence of reason. That was Aristotle's method. It was not Bacon's, and Bacon's method has been the English method for some three hundred years—at least, that is the common belief. Fortunately, Bacon's method was not in fact followed, or we should not now be enjoying the fruits of many a great investigator's work.

Aristotle wrought over a large field and was at little pains to verify his facts, though he was a more careful observer than people commonly give him credit for. But there are signs that people are wearying of fact-collection, and, not knowing exactly what to do with the fact-heaps gathered on Bacon's system, are "going back to Aristotle" and giving a due recognition to the philosophic side of the process.

They do not know much of science, these early Humanists, and to us the absurdity is plain of seeking information in Greek writers two thousand years back, instead of looking at the world as it lay before them; but, at

least, it can be said that the masters at whose feet they sat were intellectual giants.

This too can be said, that the Renaissance influence directly encouraged such matter-of-fact things as anatomy and botany and clinical medicine—to confine the examples to medicine alone. Things were to be studied at first hand ; and of all the things to be studied foremost were placed man and what he took delight in—poetry and art and eloquence : there was no dominating desire to further man's creature comforts.

One characteristic of the English Humanists stands out in the clearest relief—their altruistic attitude. Of what they had received they desired to give. Linacre founded a College of Medicine and gave of his wealth to found lectureships at Oxford. Caius, who as a young man had known Linacre, and wrote his epitaph, founded a College. Colet, Linacre's great friend, founded the first really free High School in England. They believed in education.

In or about 1510 Linacre gave up public life and became a priest. Archbishop Warham collated him rector of Mersham in his native county. He was then about fifty years old. In his private letters Linacre says he wished to gain leisure for literary work. Probably he felt his duty lay in that direction rather than in the amassing of wealth.

Some biographers comment unfavourably on the rapid succession of ecclesiastical preferments he received: rector of Hawkhurst, Kent (1510) ; prebend of St. Stephen's, Westminster (1517) ; rector of Halsworth, Devon (1518) ; precentor of York Cathedral (1519). Some of these benefices he resigned shortly after their bestowal. Dr. Payne (to whose article in the *Dictionary of National Biography* I am indebted for many references) thinks the object of the presentations and resignations was to obtain money from the next aspirant for the position—" a procedure not uncommon then, and

not unknown now," he adds. I am slow to make this assumption. There is no evidence for it other than the dates given. As Professor Walsh remarks, the cost of induction must have been heavy, and it is at least as probable a theory that Warham wished to get the influence of Linacre's known organizing capacity to bear on the affairs of establishments in need of reform. Linacre was in no need of money; and, again, money to him was only in trust for good purposes.

Archbishop Warham, the patron of most of the benefices, was likely enough to give a poor scholar preferment—he gave Erasmus the benefice of Aldington in Kent; but there personal kindness would stop. He was not, like Wolsey, a great courtier, but he was more—he was a great Churchman. "He had sufficient time for a scrupulous performance of the accustomed exercises of prayer, for the almost daily celebration of the Mass, for twice or thrice hearing Divine Service . . . for the visitation of churches when regulation . . . was needed." This is Erasmus's description, and Erasmus was quick enough to detect faults. The last words support the suggestion that Linacre was expected to help in administration.

Of Linacre's personal character the element which his contemporaries emphasize is his hatred of deceit. He was, to quote his epitaph written by Caius—himself one of the foremost Englishmen of the sixteenth century, who did not use words lightly—"one that hated above all deceit and underhand work, a true friend, dear to rich and poor alike." Until more direct evidence of traffic in benefices is offered, we may safely fall back on this contemporary testimony to the integrity of Warham and Linacre. Froude says that Archbishop Warham died poor, "left scarce enough to bury him."

In a lecture by Professor Dewey delivered at Columbia University, New York (1910), he speaks of "Protestant Humanism re-editing Catholic Scholasticism." The relation between Protestantism and Humanism is too

big a question to discuss here, but one must protest against the assumption that it was exclusively a Protestant possession. There were Catholic Humanists before the new creed had gained many adherents; the names of some have been alluded to, and stress laid on one characteristic—their zeal in educational reform. The College of Physicians was, as we have seen, Linacre's work. "Caius keeps his memory green" in Caius College, Cambridge. Colet, Linacre's great friend, as has been already stated, founded the first really free school in London—St. Paul's. "Despite (!) his training at Oxford for an ecclesiastical career under Popish dispensation," Howard Staunton (*Great Schools of England*) says of Colet that "he was from his youth one of the most zealous, able, and influential promoters of renewed life in religion and letters."

I do not know what manner of man this writer would expect to see as the normal result of "a training at Oxford under Popish dispensation," but he is very frank in his appreciation of Colet, and his criticism expresses fairly what Colet and the other Catholic Humanists tried to do. Colet founded St. Paul's "in the yeare of our Lorde 1512 . . . in the honor of Christe Jesu in childhood (*in pueritia*) and of his blessed moder Marie." "There shall be taughte in the Scole children of all Nations and Contres." "A childe at the first admission, once and for ever, shall pay 4d." It was a liberal scheme. We may note in passing that the school work of the Renaissance scholars, in so far as it was early English and Catholic, and bound by many ties to the universities, may claim direct descent from Waynflete and Wykeham, Bishops of Winchester. From 1380 (Winchester) to 1509 (St. Paul's) there was constructive work going on in English education which has left its mark on English character. The Protestant Renaissance cannot be denied, but the existence of a Catholic Renaissance and its progressive and liberal spirit must not be ignored.

Something may now be said of Linacre's intimate life. The fine portrait in the Royal Galleries (said to be the work of Quentin Matsys), which is our best presentation of him, shows a grave enough demeanour, as befits a physician dealing with the ills that flesh is heir to; but there are lines about the mouth that suggest the possession of some share of the divine gift of humour and its accompaniment of kindly tolerance of other people's ways.

He saw good company—Lorenzo de' Medici and the Tudor kings and princesses,—but perhaps never better than that which he met at Stepney—a name which nowadays does not suggest rural surroundings, but in the sixteenth century it was a pleasant suburb of London.

Colet was rector of Stepney, and More, Erasmus, and Linacre were there often as his guests: we may be sure there was pleasant talk as well as mention of grave matters. The gaiety of More—not to be suppressed even when mounting the scaffold to suffer for conscience' sake; Erasmus' scholarly humour, and the unfailing gentleness of disposition that made him friends in the most unlikely quarters; found an admirable foil in Linacre's dignified gravity.

We may hear Erasmus asking the sympathy of the company in so far as he had not the benefit of Linacre's advice on his last trip across the Channel, or that a prescription of the famous English physician had failed to be interpreted properly by a foreign practitioner. Colet's mother, who held Erasmus in high favour, is of the party. This is what Erasmus says of her: "I knew in England the mother of John Colet, a matron of singular piety. She had by the same husband eleven sons and as many daughters, all which hopeful brood was snatched away from her except her eldest son, and she lost her husband far advanced in years; she herself being come up to her 90th year looked so smooth and was so cheerful, that you would think she had never shed a tear nor brought a child into the world. That which supplied a

woman with so much fortitude was not learning, but piety toward God."

Of Erasmus himself it may be noted that the work *De Amabili Ecclesiæ Concordia*, published in 1533, three years before his death, in which he gives us his last word, "Without the unity of the Church there can be no Christian peace," may be taken as fixing his position. In that book, it has been said, is the soul of Erasmus. This is what Erasmus says of others of the party:—"When Colet speaks I might be listening to Plato. Linacre is as deep and acute a thinker as I have ever met with. Grocyn is a mine of knowledge. And Nature never formed a sweeter and happier disposition than that of Thomas More."

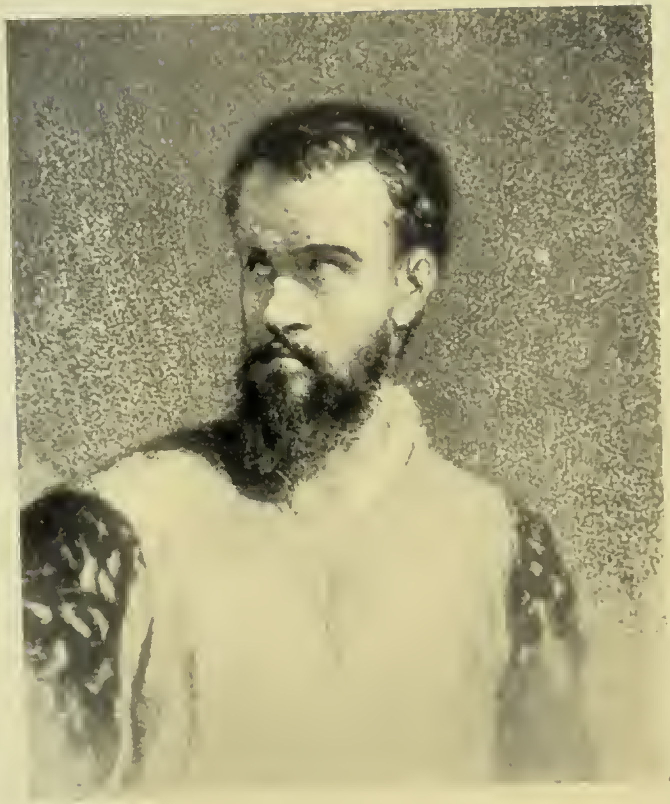
The rectory garden was evidently a feature of the scene, for we are told that Madame Colet's strawberries were among the first imported from Holland, Erasmus's gift, and that the damask roses introduced into England by Linacre were another part of the hostess's possessions.

It is a pleasing picture, that Stepney meeting—looked back on, perhaps, in less happy times with a keener feeling of the peace and happiness that were all the more real because unconsciously enjoyed.

Linacre's writings are chiefly translations from Greek (direct from the manuscripts) into Latin. They include the works of Galen in many volumes—that on Temperament is said to have been the first book printed in Greek type in England. The dedications are to Wolsey, to Henry VIII. (by command), to Warham. Erasmus speaks of the extreme fastidiousness of Linacre as to publishing ill-prepared matter; probably this accounts for the non-appearance of the translation of Aristotle which Erasmus tells us had been made in Latin as clear and thorough as Aristotle's Greek. There was also a series of grammars, one for St. Paul's School (the basis of Colet's), another for the Princess Mary, *On the Structure of Latin Speech*—a standard work for many years.

Aldus speaks of the severe classic beauty of Linacre's style, and hopes the Italian scholars may profit thereby.

Linacre died on 20th October 1524, and was buried in the old Cathedral of St. Paul's, London. No memorial marked his grave till 1557, when Caius wrote the epitaph already quoted (p. 12).



ANDREAS VESALIUS

ANDREAS VESALIUS

(1514–1564)

BY

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Properly to appreciate the career of Vesalius, who so eminently deserves the title of Father of Modern Anatomy which subsequent generations, and especially our own, have generally accorded him, it is extremely important to recall that his life falls in the midst of the period that is usually spoken of as the Renaissance. He is a striking example of the power of accomplishment that came to so many at this wonderful time. Born in 1512, dying in 1564, the year of Michel Angelo's death and Shakespeare's birth, his life occurs just in the midst of the great period. Among his contemporaries, besides Michel Angelo, were such men as Leonardo da Vinci, Raphael, Titian, Ariosto, Rabelais, Montaigne; while Camoens, and even Tasso and Cervantes, as well as Lope de Vega, were growing to manhood before his death. There are many others in his time whose names we scarcely recall in this company, though had their careers fallen on any other time they would seem bright particular stars because of the brilliancy of their accomplishment.

The names that we have mentioned are those which are likely to occur whenever a reasonably well-read person thinks of the Renaissance. They are the painters, sculptors, architects, and writers of the time. For most

people the Renaissance represents a rebirth in arts and letters. Often it is forgotten that there was a corresponding rebirth in science, both theoretic and applied, at this time. There were great mathematicians and astronomers, distinguished physicians who did ground-breaking work in chemistry, and above all in anatomy; and there were workers in other sciences whose names are fondly remembered by those who follow them in the after time, and who have interest enough to know something about the history of their specialty. Such names as Copernicus, Paracelsus, Vesalius, Columbus, Varolius, Eustachius, and Cæsalpinus, with others that might readily be mentioned, show how truly this was a time of rebirth in science as well as in arts and letters. Only too often this is forgotten in the persuasion that science, in the sense of observations on physical nature and the deduction of the laws and principles that underlie physical phenomena, is a comparatively modern development. Anyone who knows Renaissance science well is not likely to think of much of our modern science as new in any proper sense of the word.[1]

There are some who are inclined to take the word Renaissance in a certain literal signification, and to presume that it represents a rebirth out of nothingness, as if the great movement of the time sprang into life where

[1] The rather interesting reflection has been made that in all Europe at the time of the Renaissance there was altogether a population of not more than 40,000,000 of people. In our Western civilization, which corresponds to the Europe of that time, we have probably 500,000,000 of people. If there is any law of progress in humanity, then we ought to have at least a dozen times as many great men as they had during the sixteenth century. Genius was so common at that time that, as James Russell Lowell suggests, almost any family might expect to have an attack of it, just as it might expect to have an attack of measles. It would be rather difficult, however, to find not a dozen times, but even half as many great men whose work will influence the future as Renaissance genius did. Of course it is well understood that the occurrence of genius follows no law, but the reflection is made as illustrating how superficial is the conviction which presumes constant progress in mankind.

there had been none before. The mistake is much less common than it used to be, but it still exists in many minds. Nothing could well be less true than that there were not great human achievements during the centuries preceding the Renaissance. I am one of those who incline to think that the thirteenth century, because of the diffusion of its greatness among a wider circle of mankind, must be considered as having achieved even more than the sixteenth. In the century immediately preceding the year 1500, however, there is abundant evidence of fine productivity in art and letters, and also in science. Cusanus and Regiomontanus, who did such fine work in mathematics and astronomy—Cantor in his History of Mathematics devotes more than a score of pages to each of them,—such men as Berengarius, the anatomist who first described the appendix, John de Vigo, who wrote the first important text-book on gun-shot wounds, and John of Arcoli, who discussed the filling of teeth and many more supposedly modern inventions in dentistry, are only a few typical examples of the men who in the later fifteenth century were laying deep and strong the foundations on which Renaissance genius was to build a magnificent structure of science in the great sixteenth century period.

Italy had been for centuries before this time, and continued to be for at least two centuries after it, the home of post-graduate work. We are rather proud of our opportunities for post-graduate work in the modern time, and sometimes forget that enthusiastic students have always sought and found special facilities for pursuing their studies even though they had to go to a distance to obtain it. During the latter half of the nineteenth century Germany has been the home of post-graduate work, especially in the sciences. During the early part of the nineteenth century, France, where such men as Ampère, Dumas, Lamarck, Cuvier, Geoffroy Saint-Hilaire, and Laennec, were doing their work, was

the Mecca for earnest students from other countries. There are few, however, apparently who realize that for six centuries before that time Italy was almost constantly the magnet to attract students desiring to have ampler facilities for special study than their educational institutions at home had been able to supply them with, and who wished to bring back new information and incentive for work to their own country. At the end of the eighteenth century, such men as Galvani, Volta, Beccaria, Spallanzani, and Morgagni were teaching there, and, before that, for half a millennium the world teachers had been at the Italian universities.

Just before the beginning of the sixteenth century Linacre had gone from England to Italy in order to complete his education in medicine and the humanities. When he crossed the Alps on his return into his own country, he built an altar on which, in true classic spirit, he made burnt offerings at that point on the road from which he got the last glimpse of the fair Italy that had been his *alma mater studiorum.* Students from many other parts of Europe also went to Italy about and after this time, because it was realized that the best opportunities for advanced work were there to be found. This was as true in science as it was in the humanities. Copernicus had gone there in order to complete his education in mathematics, astronomy, and medicine. When students came from distant Poland and England, it is easy to understand that there must have been many from the intervening countries.

Among these, within the ten years after the middle of the first half of the century, came a young man from Belgium, by name Vesalius, who, after having exhausted the possibilities of study in anatomy in his native country, had gone to Paris, and, disappointed there, had then proceeded to Italy. Here he was, in the course of a few years, before he was thirty years of age, to revolutionize the study of anatomy, and a little later to publish a great

text-book illustrated by plates of actual dissections that is one of the world's bibliographic treasures.

In Italy he was brought in contact, either personally or through his writings, sometimes indeed as fellow student, with a set of men worthy to be his colleagues. At the middle of the century, when he had completed the best part of his work, he had had personal relations of the most intimate kind with such men as Columbus, who succeeded him in the professorship of anatomy at Bologna, and to whom we attribute the discovery of the circulation of the blood in the lungs; Varolius, whose name is attached to the *pons* in the brain, though he died at the early age of thirty-two; Eustachius, after whom several structures in the human body are deservedly named, and whose studies of the anatomy of the head are still classics; Cæsalpinus, to whom the Italians rightly attribute the discovery of the general circulation, for his works contain a complete description of it a full generation before Harvey's time; and Fallopius, whose name is attached to the Fallopian tubes. There are others whose names will not be forgotten in the history of medicine who were doing work in the Italian universities of the time, but these are the most distinguished. One other, at least, we should name, John Caius, Vesalius's fellow student and room-mate at Padua, who returned to England to introduce the practice of dissection there, and later on to found Caius College, Cambridge.

This is the background on which the life of Vesalius must be seen if it is to be properly understood. He was a product of the Renaissance, born in Belgium, educated at the University of Louvain, and then trained for his great life-work in anatomy in Italy. There are writers who have said that there was Church opposition to dissection at this time. It has been rather the custom[1] to hold up even Vesalius himself as a "Horrible Example" (capitals are required rightly to emphasize the

[1] *See* White, *Warfare of Science with Theology*, vol. ii. 54, *et seq.*

expression as some would use it) of Church persecution in this matter, and emphatic assertions are made that whatever dissection he did had to be accomplished in secret and in fear and trembling of the Inquisition, the Holy Office, the ecclesiastical authorities, etc.

Some who realize how nonsensical is such talk in the light of the realities of the history of anatomy and the wonderful development of that science in the Italian universities of this time, declare that Vesalius obtained his opportunities for dissection in spite of the ecclesiastical authorities, because Padua was in the Venetian territories, and the Venetian Senate, at this time in opposition to the Popes, refused to enforce ecclesiastical decrees. This statement is even more nonsensical than the other, however, because it represents a definite far-fetched endeavour to bolster up a bad case that can only have been dictated by anti-clerical zeal. Dissection was practised at all the Italian universities at this time, and nowhere more than at Rome itself.

Vesalius taught not only in Padua, but also in Bologna. That city was at this time in the Papal States. One of his great predecessors at Bologna, Berengar of Carpi, first to describe the appendix, had made many dissections there. Columbus, who succeeded Vesalius as lecturer in Padua and was professor at Bologna, was afterwards called to Rome and did many public dissections there. When his great text-book of anatomy was published, it was dedicated to Pope Paul IV. Eustachius, Varolius, Cæsalpinus, and Piccolomini, who are the great writers on anatomy, the ardent dissectors of this century after Vesalius and Columbus, were all Papal physicians and professors of anatomy at the Papal medical school. They had made their reputations by dissection work in other universities, and then were called to the medical school at Rome because they were considered the best candidates for the good work the Popes wanted done there. At the time when Vesalius is sometimes represented as

doing his dissections in fear and trembling, Columbus at Rome was making autopsies on many distinguished ecclesiastics, cardinals, archbishops, and bishops, in order to determine what they died of. We have the protocols of these autopsies. Many of his dissections were done publicly and were attended both by prominent ecclesiastics of Rome itself and by others who were visitors *ad limina.*

For those who think that there was any ecclesiastical opposition to dissection at this time, the following paragraph written by Dr George Jackson Fisher in his *Historical and Bibliographical Notes for the Annals of Anatomy and Surgery* (Brooklyn, 1878–1880), will be illuminating. Dr Fisher, I may say, was a well-known collector of old medical books, a special student of the history of medicine in these times, and was looked up to as probably one of the best informed of our generation in the history of dissection. He said :

" The fame of Columbus as an anatomical teacher was exceedingly great and widespread. Students were attracted to the universities where he professed, from all quarters and in large numbers. He was an ardent student of his favourite science, and was imbued with the genius and enthusiasm of an original investigator. He was not satisfied with the critical examination of mere structure, but extended his researches into the more subtle, difficult, and important investigation of the physiological function. He has been most aptly styled the Claude Bernard of the sixteenth century. The work of Columbus is a masterpiece of method and purity of style, as well as on account of its richness in facts and observations. He spent over forty years in these studies and researches. *He dissected an extraordinary number of human bodies. It must have been an age of remarkable tolerance for scientific investigation, for in a single year he dissected no less than fourteen bodies.* He also entered the crypts and catacombs of ancient

churches, where the bones of the dead had been preserved and had accumulated century after century, and there, with unwearied care, he handled and compared over a half-million of human skulls."[1]

The only difficulty with regard to dissection in Vesalius's time was the procuring of bodies for the purpose. This was not due to religious scruples, but to the very human feeling of objection to having the body of a friend, and above all of a relative, subjected to what seemed an indignity. In English-speaking countries, at least, we are very familiar with that, for we are only just beyond the time when it hampered the development of anatomical science and teaching very much. There is a speech of Macaulay in the English Parliament, made some seventy-five years ago, in which he pleads for the passage of a bill providing anatomical material. In the United States, two generations ago, many of the bodies

[1] Probably the most interesting historical commentary on this whole discussion with regard to the supposed ecclesiastical prohibition or limitation of dissection at this time, is to be found in the fact that we have abundant evidence in sketches from their hands that all of the great artists of the early sixteenth century did dissections, and seem, indeed, to have done them very freely, or had the chance to study them without stint. Michel Angelo made special studies in the anatomy of muscles after dissection. Raphael, at the height of his fame as artist, turned to dissection in order to learn human anatomy better. Titian, according to one tradition which is not however, certain, made the plates for Vesalius's Anatomy. If not by Titian they are by one of his pupils, a distinguished artist. The main reason for an artist taking up such work was to have the opportunity to study the dissected specimens which had been made for the illustrations. Leonardo da Vinci made special studies in the anatomy of man and the horse with a large number of dissections. He is said to have written a short treatise with regard to dissection and made a series of plates of mesial, sagittal sections of the male and female bodies. In other countries the same custom as to dissection for artists was established. Albrecht Dürer made a series of dissections, and French artists did the same thing. If artists, all of whom were in intimate relations with the ecclesiastics of the time, made dissections thus freely, there surely could have been no difficulty for physicians to do so or to obtain all the material that they cared to use.

for dissecting purposes were obtained by "resurrections." As late as twenty-five years ago that practice still obtained in some States. Almost needless to say, there were no religious elements in the opposition to providing anatomical material in the modern time, nor, as we have pointed out, were there in Vesalius's time. At both periods educated clergymen and the ecclesiastical authorities were ranged on the side of enlightened privilege in this matter.

After clearing away some of the false notions with regard to his times, which have been widely prevalent and marvellously persistent in spite of the absolute lack of truth in them—anti-religious prejudice is always particularly hard to overcome or reason with,—it will be easier to understand the career of the man who so deservedly has been called the Father of Modern Anatomy.

Andreas Vesalius, or Wesele, as his name ran in his native Low Dutch, was born in Brussels, the capital of what was then called the Duchy of Brabant, in the year 1514 or 1515. The doubt with regard to the date is due to the fact that his birth took place on the night of December 31, or possibly at a morning hour of January 1, 1515. The family name is derived from the district of Wesel in Cleves in which his ancestors had formerly resided. Vesalius's ancestors had for many generations been distinguished in various departments of science, medicine, and mathematics. His great-great-grandfather had translated some treatises of Avicenna, and especially had succeeded at considerable cost in having copies made of certain Arabic medical works. His son, Vesalius's great-grandfather, occupied the position of physician-in-ordinary to Maria of Burgundy, the wife of the German Emperor, Maximilian I., the distinguished patron of letters in the Renaissance period. He lived to an advanced age as a professor of medicine in Louvain. From this time Vessalius'

family always continued in official medical relation to the Austro-Burgundian ruling family. His grandfather took his father's place as physician to Mary of Burgundy, and wrote a series of commentaries on the aphorisms of Hippocrates. Vesalius's own father was the physician and apothecary to Charles V. for a while, and accompanied the Emperor on journeys and campaigns.

With an heredity like this, it will not be surprising to find that Vesalius was tendered and accepted the post of royal physician to Charles V. when he had reached the age and attained the prestige to fit him for such a responsible post. Recent biographers seem to think that the main reason why Vesalius accepted the position offered by Charles V. was that he was disgusted with the persecution of the Galenists and the ecclesiastics in Italy, and that he wished to escape from such uncomfortable surroundings. As a matter of fact, however, he and his family must have looked forward for many years to the possibility of his taking this position. After all, there are very few men so deeply interested in science as, for its sake, to refuse advantages, financial and personal, such as were thus held out to Vesalius.

From his earliest years he was known for his tendency to be inquisitive with regard to natural objects, and while still a mere boy his anatomical curiosity manifested itself in a very practical way. He recalls himself in later years that the bladders with which he learned to swim, and which were also used by the children of the time as play toys for making all sorts of noises, became in his hands objects of anatomical investigation. Anatomy means the cutting up of things, and this Vesalius literally did with the bladders. He noted particularly that they were composed of layers and fibres of various kinds, and later in life, when he was studying the veins in human and animal bodies, he was reminded of these early observations, and pointed out that the vein walls were made up of structures not

unlike to, though more delicate than, those of which the bladders of his childhood days had proved to be composed.

Vesalius's early education was received entirely in his native town. There were certain preparatory schools in connection with the University at Louvain, and to one of these—called Pedagogium Castri, because of the sign over the door, a fort—Vesalius was sent. Here he learned Latin and Greek and some Hebrew. How well he learned his Latin will be readily realized from the fact that at twenty-two he was fitted to lecture on anatomy in Italy in that language. His knowledge of Greek can be judged from the circumstance that he could translate Galen at sight, and was known to have corrected a number of errors in translations from that author made by preceding translators. To those who know the traditions of that time in the teaching of the classic languages along the Rhine and in the Low Countries, these accomplishments of Vesalius will not be surprising. They knew how to teach in those pre-reformation days, and probably Latin and Greek have never been better taught than by the Brethren of the Common Life, whose schools for nearly a hundred years had in the Low Countries and Rhenish Germany been open to the children of all classes, but especially of the poor. Erasmus, Thomas à Kempis, Alexander Hegius, Agricola, Cusanus, were the products of these schools. Other schools of the region could scarcely fail to be uplifted by such educational traditions.

Altogether, Vesalius spent some nine years in the Pedagogium. Then he seems to have spent five further years in the University itself, where he studied philosophy and philology as well as languages and literature. It may be noted, as illustrating how a student will find that which appeals to him even in the most unexpected sources, that Vesalius took special interest in certain treatises of Albertus Magnus and Michael Scotus which

dealt with the human body in the rather halting, incomplete way of the medieval scholars, and yet with a precious amount of information that this scientifically inquisitive youth eagerly drank in. More interesting for Vesalius himself were certain studies undertaken entirely independently of his university course. One of his biographers declares that he dissected small animals, rats and mice, and occasionally even dogs and cats, in his eagerness to learn the details of anatomy for himself and at first hand.

The educational traditions of the University at Louvain were the very best. At the end of the fifteenth and the beginning of the sixteenth centuries the University had probably more students than any other university in Europe, except that of Paris, and it seems likely that the number in attendance during the first half of the sixteenth century was always in excess of 5000. The University was especially famous for its work in jurisprudence and philology. The faculty of theology, however, was considered to be one of the strongest in Europe, and Louvain was always strongly opposed to the progress of Lutheranism, and was one of the bulwarks of the Catholic Church. Such the University continued to be for several centuries, and though it was suppressed in Napoleon's time, it was refounded towards the end of the first quarter of the nineteenth century, and has nobly continued the traditions of the past. It was while professor at Louvain that Theodore Schwann published the little book which is the foundation of the cell doctrine and the very corner-stone of modern biology.

After graduating at Louvain in philosophy and philology, Vesalius went to Paris to study medicine. At this time at Paris, Sylvius, after whom one of the most important fissures of the brain, the sylvian, is named, was not only teaching anatomy in a very interesting way, but was also providing opportunities for original research in anatomy in connection with his own investiga-

tions. The interest that his teaching excited may be gathered from the fact that over four hundred students were in attendance at his lectures. Besides Sylvius, Günther of Andernach in Germany was also teaching in Paris, and with both of these distinguished professors, Vesalius, owing partly to the deep interest he displayed in the subject, and partly to the influence he was able to exert because of letters of introduction from court physicians, became intimately associated. It was at Paris that Vesalius first showed the practical temper of his character and his intense desire to learn anatomy at first hand. The dissections used to be done by the barber surgeons, rather ignorant men, who knew little of their work, but the demonstrators in anatomy at this time considered it beneath them to use the knife themselves. Vesalius, however, insisted on doing his dissections for himself.

Not being able, however, to secure facilities for study and opportunities for original work such as he desired, Vesalius did not remain long at Paris, but returned after a year to his home in the Low Countries. At Louvain he continued his anatomical studies, finding it difficult enough to procure human material, but using such as might come to hand. The story is told of his first successful attempt to get a complete skeleton. A felon had been executed just outside the walls of Louvain some time before, and his remains, according to the legal regulations of that time, were allowed to swing in chains on the gibbet until the birds had eaten his flesh and the wind and rain had bleached his bones. Needless to say, these bones were a great temptation to Vesalius. Finally, one night he and a fellow student stole out of the town and robbed the gibbet of its treasure. In order to accomplish their task—no easy one, because the skeleton was fastened to the beams of the scaffold by iron shackles—they had to remain outside the gates of the city all night. Unable to carry it all back at one

time without great risk of detection, they buried it and later removed it piecemeal. When they had finally assembled the parts again, it was exhibited as a skeleton brought from Paris.

Louvain at this time was not the small university town that it is now, but one of the most important towns in Europe. It has been calculated that it had nearly 200,000 inhabitants, and its business relations were very extensive. Vesalius, however, was not satisfied with his private studies, and even this important city could not supply him with facilities for his desired researches. Opportunities for anatomical investigation were known to be abundant in Italy, so it was not long before he made his way there. At first Vesalius seems to have spent some time in Venice, where he attracted considerable attention by his thoroughly practical anatomical knowledge and independent mode of thinking. After only a short period in Venice, however, he proceeded to Padua, where he spent some months in preparation for his doctor's examination. It is known that, having completed his examination in the early part of December 1537, he was allowed within a few days to begin the teaching of anatomy, and, indeed, was given the title of professor by the university authorities. The next six years were spent in teaching at Padua, Bologna, and Pisa, and in fruitful investigation. Every opportunity to make dissections was gladly seized, and Vesalius' influence enabled him to obtain a large amount of excellent anatomical material. He began at once the preparations for the publication of an important work on the anatomy of the human body. This was published in 1543 at Basle, at a time when its author was not yet thirty years of age. It is one of the classics of anatomical literature. Even at the present day it is often consulted by those who wish to see the illustrative details of Vesalius's wonderful dissections as given in the magnificent plates that the work contains. In the early editions

it has become one of the most precious of medical books, and is eagerly sought for by collectors.

Vesalius's observations related to the anatomy of every portion of the body. His work is founded on his own studies, and only in minor points does he accept the authority of previous writers. It would not be hard to show what a very great difference this made in anatomical teaching. Roth has taken the knee-cap and the sternum (breast-bone) as examples of what was known before and after Vesalius. Vesalius's teaching with regard to both of these is the basis of our modern teaching, and very little has been added to his descriptions. His predecessors had made egregious blunders, as a rule, because of dependence on animal dissections rather than actual observations on human bodies. It must not be thought, however, that Vesalius's observations concern mainly the bones, and above all not exclusively. He added just as much to the exact scientific knowledge of the soft parts. This is particularly noticeable with regard to feminine anatomy. Female bodies had been particularly hard to procure for dissection purposes, and there were many erroneous teachings with regard to feminine anatomy. Vesalius himself found it difficult to obtain female bodies, for women do not often die as strangers in cities distant from their homes; but we know that he dissected at least six of them (probably more), and he revolutionized the teaching in this department of anatomy.

If there are any who think that fine book-making is a modern development, and that our text-books of the modern time must, by the very fact of the accumulation of information as to how best to illustrate teaching, be better than those of three centuries and a half ago, he only needs to have a look into Vesalius's great text-book of anatomy, published in 1543, to be convinced that probably nothing better than this has ever been provided for teaching purposes in anatomy, so far as devo-

tion to it to make it technically perfect may go. The anatomical illustrations are finely made. They are real works of art. Skeletons are not represented in the conventional standing or pendant position, but in a number of attitudes that illustrate bone functions and relations very well. The same thing is true for plates illustrating myology. The attitudes of the muscle-clothed skeletons are full of expression and make very clear the functions of the various muscles. There is a tradition that these fine plates, or at least the sketches for them, were made by Titian, but it seems much more probable that they were made by Titian's distinguished pupil, Kalkar, whose name occurs in such variant forms as Calcard and Calcarensis. The difference between these plates and those of preceding authors on anatomy is immense, but it is not greater than that between Vesalius and many writers on anatomy long after his time.

In his second edition Vesalius made a great many changes, all of them meant more fully to develop anatomical knowledge. This second edition, issued in 1555, omitted many autobiographical details, and, as it was this edition that was most used, until comparatively recent years there was apparently a dearth of information with regard to its author. As Vesalius grew older, however, he had learned to care less about himself and more about his work as a contribution to science. He corrected many passages even in punctuation, and made a new index; repetitions were eliminated, and the order of the contents improved. He dispenses with a number of attacks that he had made on Galen, apparently with the idea of eliminating controversial matters. He described a new instrument with which he penetrated the hard bones in order to study them. He discussed the possibility of the existence of valves in the veins, but thought that the appearances which had been described were only thickenings of the walls. He added a number

of details in regard to the genital regions and the anatomy of the embryo and accompanying parts. He was evidently labouring to make his text-book just as valuable as possible for serious students of anatomy, without any question of its cost or of the labour involved for him.

Vesalius has taken advantage of the opportunity presented by his figures of skeletons to teach certain ethical lessons. Two of the plates, for instance, one in the Fabrica and the other in his Epitome, represent skeletons standing in a pose of meditation beside a pedestal, the skull resting on the left hand, the elbow on the pedestal, while beneath the other hand, the subject of the meditation as it were, is a second skull. On the base of the pedestal are the words, in Latin, " Vivitur ingenio caetera mortis erunt."

The plate in the Epitome has practically the same pose for the skeleton, but on the pedestal are the words " Solvitur omne decus leto, niveos que per artus. It stygius color et formae populatur honores."

For ten years more Vesalius devoted himself to his favourite studies in anatomy and physiology, and to the application of his discoveries to practical medicine and surgery. He was summoned in consultations on all sides, and was evidently considered one of the greatest medical practitioners of his time. This is a side of his character and development that we cannot help but think his biographers have missed, when they have assumed that the reason for his acceptance of the post of royal physician to Charles V. was the result of discouragement in his purely scientific studies by the followers of Galen. No little sympathy, indeed, has been wasted, to our mind, on this phase of Vesalius's career, since there seems no doubt that he gladly accepted what was after all the best possible opportunity, in his time, for the pursuit of the clinical phases of medicine in which he had become so much interested.

All his life Vesalius had been interested in the practical side of medicine and surgery. While the making of anatomical observations was his special life-work, and while he never missed an opportunity to make them, the applications of his discoveries seem always to have been uppermost in his mind. There is scarcely a document relating to his years of investigation, especially in Italy, which does not bear evidence of his abiding interest in the practical side of medicine. To have the opportunities that would be afforded then by the important post offered him by Charles V., who, it must be remembered, was at that time the monarch of Spain and the Netherlands, as well as the Emperor of Germany, the ruler, in a word, of most of the civilized world, for Spain dominated the greater part of America also, was just what Vesalius most desired.

Some of the practical work that he had been doing in Italy was of the greatest importance. He worked out the interesting clinical phenomenon that the spleen is likely to be affected whenever there is any liver disturbance. He pointed out that this is especially true with regard to atrophic conditions of the liver. Enlargement of the spleen in these cases seemed to Vesalius to be compensatory, and he suggested the thought that the spleen might perhaps take up some of the liver functions.

Very early in life he recognized the fact that the lower part of the thorax might be very much narrowed by the wearing of a corset, and that this interfered with the functions both of the liver and of the lungs. This is said to be the first time that attention was called to this particular evil effect of wearing the corset, and indeed the observation is supposed to be much more recent. It serves to show very clearly how practical was Vesalius' view of things.

After his studies in normal anatomy, he devoted much time to pathological anatomy and never let the patho-

logical side of things escape him. There was even question of his writing a text-book on pathological anatomy, though this was never completed, nor were his notes apparently of sufficient value to deserve publication after his death. There are many illustrations of his watchfulness of the significance of pathological conditions that might be noted. Even a few of them will emphasize his interest in clinical medicine.

The best evidence of the practical nature of Vesalius's devotion to medicine is to be found in his observations on aneurisms. We have altogether the notes of six cases of aneurisms observed by him. He was the first in the modern time to call attention to the spontaneous development of this lesion and to study the methods by which its presence could be recognized. The Greek authors had studied the condition, but Vesalius distinctly advanced the knowledge to be found in medical literature up to his time. He was the first who ever recognized an internal aneurism on a living subject, and the diagnosis that he made in this case remained unique of its kind for over one hundred years.

If in addition to this we recall the breadth of his interests in medicine, his place as a physician, in the fullest sense of the word, will be better understood. In Paris we hear of his visiting the leper houses; in Italy he made many observations on internal diseases and practised surgery. He even succeeded, as we shall see, in finding opportunities for the special study of parturition. No moment passed without his thinking of the practical side of medicine. Even his text-book of anatomy has many practical observations, and his descriptions of the joints, the muscles, the organs of special sense, the pelvis, and the female genital organs, all demonstrate that fact.

The most interesting evidence, from a modern standpoint, that we have for Vesalius's recognition of the fact that anatomy must be developed as a practical science,

is to be found, first, in his inquiries with regard to the history of the persons whose bodies came to him for dissection whenever anything out of the common was discovered in them, and, secondly, his study of anatomical relations on living subjects. For instance, when he found in a dissection that the optic nerves were uncrossed, he made inquiries as to whether the person during life had suffered from double vision. When in the body of one of the rowers of a papal vessel he found a second biliary canal emptying into the stomach, he made inquiries as to whether there had been during life any vomiting of bile. There had been none, and Vesalius notes that fact, though Galen had declared that in such cases biliary vomiting must be frequent.

His observations on living subjects began with himself and the eruption of a wisdom tooth. He experimented with his respiratory movements, and enumerates the sensitive points along the nerves that run to the fingers. Certain of his friends were able to move their ears, and he made special observations on them. One of his students had such control over the manual muscles that he could hold water on the back of his hands. He notes that a wandering performer in Padua was able to lift a twenty-five pound iron bar with his teeth and throw it thirty-nine feet behind him. Vesalius himself controlled the weights and measurements in the test. He had made observations on curiously shaped skulls in the Netherlands, in Venice, and in Bologna.

One of the most interesting features of Vesalius's correspondence as it has been preserved for us, is the series of dispensations from fasting which were obtained from him by men of his time. They evidently took their obligation to fast rather seriously, since they turned to a physician for a certificate in order to justify their exemption from it. One of these, sent to the Bishop of Limoges, shows the formulæ that Vesalius most frequently used. It is, of course, in Latin.

"Whereas the Most Reverend Lord Bishop of Limoges cannot, without grave danger to his health, which has been undermined, make the Lenten fast, I, as far as in me lies, have ordered and conceded the use of flesh to his Most Reverend Lordship during this Lent. Toledo, February 18, 1561.—ANDREAS VESALIUS."

With all this interest in practical medicine it cannot be surprising that Vesalius should have accepted a post that offered him the widest possible opportunities for his talents as a physician and surgeon, for it opened up to him the best consultant practice in Europe. After Charles had abdicated in favour of his son, Philip II., that monarch continued Vesalius in his office, and proved a faithful friend to him. Some five years later Vesalius was summoned to France to attend Henry II., who had been mortally wounded in the tournament held in celebration of the dual marriages of his daughter Elizabeth with Philip and his sister Margaret with the Duke of Savoy. On Vesalius's arrival at the French court the king was beyond all medical aid. The point of a broken lance had entered his right eye, and meningitis had set in with fatal result. Vesalius returned to Spain shortly afterwards, and was called upon to treat Philip's son, Don Carlos, for an injury of the head. Poor Don Carlos, never overstrong mentally, seems after this not to have quite regained even his former mental condition, though Vesalius gained great reputation for the cure that he effected in this case. There are a number of other details of Vesalius's success as a practitioner of medicine, some of which might not count for very much in these modern days. He seems on at least one occasion to have made a prophecy as to the length of time that a distinguished patient suffering from a malignant disease would live, and, as the event justified his prediction, the fame of his knowledge went far and wide. Modern physicians know too well how few are the grounds for making such a prophecy with

anything like justifiable accuracy. But success in practice is not seldom a question of happy accident rather than of great knowledge, for medical practice is an art and not a science.

For ten years Vesalius continued in his post of royal physician, the most considered medical man in all the world of the time. Few medical investigators of his day made what they thought significant discoveries without communicating them in abstract to him, and he had many correspondents. Then came some factors not easily to be determined—whether a dissatisfaction with his occupation, a failure of health in the Spanish climate, a disagreement with his wife, a homesickness for the Low Countries, or a desire to get back to his teaching and investigation in Italy, for all of these are hinted at by his biographers, and we cannot now decide—which led to his leaving Spain, and eventually to his perishing in the island of Zante. We shall leave the discussion of these till a little later to say something of Vesalius's state of mind as regards the religious problems which were agitating the minds of his generation, because this will serve to throw light on the circumstances of his death.

The attitude of Vesalius towards religion and religious principles has met with some curious misinterpretations on the part of his biographers. Because he was a scientist and an original investigator in biology, some of his modern biographers, at least, seem to conclude from their own feelings in such matters that it would be quite impossible to imagine that he could have taken his religion seriously. They transfer the state of mind of the present time in this matter, which is by no means shared by all our greatest scientists, back to the middle of the sixteenth century. They seem entirely to forget the devout attitude of so many of the great men of that time towards the Church. Probably the men in Italy with whom Vesalius was most likely to be brought in

contact intimately, and with some of whom indeed we know that he was very closely associated, were the great anatomists and artists. Columbus, Eustachius, Varolius, Cæsalpinus, were all faithful believers in religious truth. Titian was a great personal friend of Vesalius. Michel Angelo surely knew of Vesalius's work and used some of it in his own anatomical studies. These men, at least his equals in genius, were faithful adherents of the Church, and did not hesitate to proclaim her influence over their lives. It would not be surprising, then, if Vesalius gave evidence of his faithfulness in matters of doctrine and belief.

Realizing this, it is rather hard to understand the attitude assumed by his biographers with regard to certain expressions of Vesalius. In the first edition of his famous book on anatomy, Vesalius had expressed his reverence for Galen, and had said that he considered it scarcely wise for him to disagree with this great prince of teachers. In writing to a friend some time before he published his book on anatomy, Vesalius had said that he would as soon doubt of the immortality of the soul as it is taught "in our most holy religion" (in nostra sanctissima religione) as doubt of Galen. Roth considers that this is ironically spoken, though, as a matter of fact, Vesalius never cared to set himself up in opposition to Galen,[1] and even in the last edition of his anatomy, issued in 1553, he has added very few things that differ from that old master.

[1] The more we know of Galen in the modern time the more do we learn to appreciate the admiration and the devotion to him of the medieval and Renaissance scholars. He eminently deserved it. He made mistakes, but his outlook on the whole of medicine was marvellously keen and his anticipation of some of our greatest thoughts almost inexplicable. Fresh air, rest, good food, especially milk and eggs, as the rations of consumptives, is a typical example. While his works have been translated into Latin, there is no complete edition in any modern language, and when we have modern translations we shall think almost as much of Galen as did the medieval scholars. He was for medicine what Aristotle was for philosophy.

With regard to another passage in which Vesalius expresses his agreement with Galen, even though now it is well known that Galen was wrong on this point, he also takes occasion to make a remark that seems very appropriate in the mouth of the Christian anatomist, and similar to remarks which have often been made by professors of anatomy, in Italy especially. The passage is as follows :—

"The septum of the ventricles, composed as I have said of the thickest substance of the heart, abounds on both sides with little pits impressed in it. Of these pits, none so far at least as can be perceived by the senses, penetrate through from the right into the left ventricle, so that we are driven to wonder at the handiwork of the Almighty, by means of which blood sweats from the right into the left ventricle through passages which escape human vision."

Of this passage, which seems sufficiently natural and straightforward, the late Sir Michael Foster says: "Even in this which he ventured to print, the sarcastic note of scepticism makes itself heard; but what he really thought he did not dare to put forward."

It seems almost supererogatory to protest that it is extremely difficult to see any good reason for diverting these passages of Vesalius from the plain and simple sense which they contain to that of irony. It is the modern sceptic who puts into Vesalius's words a meaning that is not justified by the context in which they stand, nor by what we know of Vesalius's life or of the circumstances in which he lived. Some of the thinkers and writers and artists of the Renaissance had rationalistic tendencies, but the geniuses of the times were almost without exception faithful in their adherence to the authority of the old Church that had stood for so much in the development of education in the preceding centuries, and whose influence was still thought to be for good even though there were, as there are always bound

to be, elements of human intolerance in the application of some of her regulations.

The circumstances of Vesalius's death have been the subject of no little dispute and of a series of varying traditions. Studied in the light of the attitude of his contemporaries to religion, they serve only to bring out the essentially religious character of the man. It is known that he resigned his position as court physician to Philip II. and made a pilgrimage to the Holy Land. On his return from this pilgrimage the vessel was wrecked on the island of Zante, and there Vesalius perished from hunger and illness. These are the facts. The reasons for the pilgrimage, however, are in dispute. According to one physician, Vesalius performed an autopsy on the body of a person supposed to be dead, but who proved only to have been in a trance. The matter was reported to the Spanish Inquisition, and Vesalius is said to have been condemned to death. This punishment was changed to the penance of a pilgrimage to the Holy Land by Philip II. For this explanation of the pilgrimage there is a letter of the year 1565. It has been doubted, however, by many subsequent authorities. The principal difficulty is that such autopsies were not allowed in Madrid, where Vesalius was at the time, because of the presence of the Court.

Another explanation is that given by the distinguished botanist, Carolus Clusius, who gathered his materials with regard to Vesalius at the Court of Madrid and in Brussels in the year 1565. According to Clusius, the Spanish climate did not agree with Vesalius, and he expressed the desire to return to the Low Countries. The king did not, however, wish to part with him. Vesalius fell ill and his life was despaired of. He made a vow to go to the Holy Land in case of his recovery, and it was while returning from the fulfilment of this vow that he died on the Island of Zante.

The story of Vesalius's vow to make a pilgrimage to

the Holy Land is quite in accord with the spirit of the times. Not a few of his contemporaries, scarcely, if any less great than himself, had done things not unlike this. It must be remembered that the spirit of the Crusades was not yet dead in Europe, and that, within a few years after Vesalius's death, volunteers were readily found in all parts of Europe to fight against the Turks in what was looked upon as a Holy War. The battle of Lepanto (1571) was fought by true crusaders. So great a contemporary as Cervantes considered it the privilege of his lifetime that he had been allowed to take part in the battle. It is from the feelings of contemporaries, as we must insist over and over again, that the spirit in which a man of a particular time does a certain action must be judged, not from the point of view of men of far distant generations, of very different temper of mind and widely diverse environment and education.

What constitutes one of the main difficulties in the acceptance to-day of the story of Vesalius's pilgrimage to the Holy Land and subsequent death, though told by the best authority, is undoubtedly the fact that modern scientific biographers find it difficult to give credence to the thought that Vesalius when ill should have made a vow to go on a pilgrimage to the Holy Land if he recovered from his illness. To certain modern views as to the efficacy of prayer, this seems unworthy of a great scientist and original thinker. A moment's consideration, however, with regard to the great men of Vesalius's time will make it appear that this spirit of belief in simple humility was very common among them. Anyone, for instance, who knows the life of Columbus, the discoverer of America, would not be surprised to find such an event in his career. Men almost, if not quite, as distinguished as Vesalius in the science of the day did not hesitate openly to express religious views that would seem quite as obnoxious or

perhaps incredible to the modern materialistic scientist, or perhaps even to those who, without losing their faith, have had it sadly dimmed by the persuasive spirit of the scientific *Zeitgeist.* The great genius Leonardo da Vinci, one of the most original thinkers of all time, dying when Vesalius was five years old, left money for Masses to be said for his soul and for candles to be burned before the altar of the Blessed Virgin. Michel Angelo's sonnet to his crucifix is a proof of a similar state of mind. Linacre, the best Greek scholar of his time, and one of the most distinguished physicians, the founder of the Royal College of Physicians in England, gave up the lucrative and highly honourable position of physician to the English king, distributed his fortune between the two universities Oxford and Cambridge and the Royal College of Physicians, and became a priest, for "the making of his soul," during the years when Vesalius was a student at the University of Louvain. Copernicus, the supreme astronomical genius of our modern times who literally gave the world a new universe, a physician as well as astronomer, always faithfully occupying his post as canon of the Cathedral of Frauenberg, sometimes exercising the duties of Chancellor of the Diocese, lived all his life as a devout minister of religion, and died after special declarations of his entire submission of all of his writings to the censorship of the Church. Erasmus, the distinguished Renaissance scholar, died at Basle not long before the time when Vesalius made his first visit to the city, and, though it is often claimed that Erasmus was in sympathy with the so-called reformers (how he would have been amused to have the term, as we understand it, seriously applied to him), it must not be forgotten that within two years before his death he had been offered a Cardinal's hat by that stern reforming pontiff, Paul III., had refused it because of his poor health, and had asked for the special blessings of the Pope for his dying hours.

The most natural explanation, then, of Vesalius's pilgrimage to the Holy Land was that it was in fact an accomplishment of the vow made while he was ill on condition that he should recover. This supreme expression of his religious faith is what might be expected from the man who had been so long in the counsels of Charles V., and, later, in those of his son, Philip II. If there had been any suspicion of the orthodoxy of Vesalius, Charles, on his retirement to a monastery, would not have been likely to have recommended him to Philip, his son and successor. Further, there is a tradition which we have already mentioned, that though Vesalius was rather anxious to leave Spain for his native land, Philip could not be brought to consent to his departure.

In a word, we have in Vesalius's life a type of the great original thinkers of the Renaissance time, men who were too broad to harbour the petty materialism of later scientists, and who remained faithful adherents of the form of Christianity with which they had been brought intimately in contact, from which they had derived many consolations in the trials of life, and to which their devotion had not been disturbed by the religious revolt in Germany. Vesalius always remained what so many of the greatest medical scientists have been, a sincere and even devout son of the Catholic Church. His death came probably in the accomplishment of a vow undertaken from religious motives, and perhaps with the idea of atoning for an unfortunate taking of human life, for which he could not be held directly responsible, but with regard to which he did not feel himself entirely blameless.

NICOLAUS STENSEN

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(1638–1687)

BY

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One of the objects of the series of which this essay forms a part is to show how baseless is the accusation that a love for and a competence in science, and a devotion to the Catholic Church, cannot possibly flourish in the same breast. Such is the accusation often made, indeed regarded as axiomatic, by many who are misled by the superficially informed writers of the myriad articles and manuals flourishing for a day like the ephemeridæ and disappearing as rapidly. As rapidly, but not always as harmlessly, for each may have left its venom in the mind of some casual reader, as the mosquito deposits its baneful legacy of malaria in the blood of its victim. As any one knows who has ever made a study of the history of science, the statement alluded to above is not only inaccurate, but ridiculously so. However, "throw enough mud, and some of it is sure to stick;" and many well-disposed persons at the present day do really believe that, if it had its way, the Catholic Church would shut up every Laboratory and Institute of Science in the world, and would place under the ban every person who made any effort to elucidate the secrets of nature. When one

asks what ground there is for supposing that the Church has always been, is, and will always remain the undying enemy of Science, the usual reply is, "Oh, everybody knows it!" or if the interlocutor is a little bit better read, and has actually heard of that astronomer, "Oh, look at Galileo!" Here we may be permitted to turn aside for one moment in order to remark that if the Church is so persistent a persecutor of science, it is somewhat strange that, during all the years through which she enjoyed so great a power, Galileo should be the one and only instance of persecution which rises to the minds and tongues of men. It is no part of the scope of this essay to deal with the case of Galileo, which has already been sufficiently treated by Fr. Gerard in a penny pamphlet published by the Catholic Truth Society; but it may incidentally be remarked that the late Professor Huxley, who cannot be accused of any leanings towards the Scarlet Woman, after having studied the matter, placed it on record that, in his opinion, "the Pope and Cardinals had rather the best of it."

Amongst the specific accusations—for though most of the accusations are general, there actually are some which are specific—which are made against the Church is that which alleges that in the person of her Supreme Pontiffs she has always set herself against the practice of Human Anatomy, and consequently, as that science is the foundation of all rational medicine, that she has done what in her lies to prevent any extension of the knowledge of the healing art, advances in which have done so much to alleviate the miseries of man in this valley of tears. With this accusation we shall deal in a moment, but before turning to its consideration we may further state that geology is a science which is supposed to be specially inimical to the Church, or rather towards which the Church is

especially inimical, from some sort of wild idea that geological discoveries conflict with her religious principles.

The life of the subject of this essay is an absolute confutation of both of these accusations. Probably ninety-nine per cent. of the persons who prattle about Religion and Science have never heard of Nicolaus Stensen, but his name and his fame, great in his own day, still shine bright in the estimation of all men of science, and the incidents of his career with some notice of the discoveries for which he was responsible may well afford food for thought on the prefatory remarks just made.

But before turning to Stensen it may be well to devote a few moments to considering the question of anatomy alluded to above. Here is a definite accusation often made in print; is there any ground for it?

For a reply to this question—a full reply—the reader may be referred to the erudite works of Professor J. J. Walsh, of Fordham University, New York, to which the present writer unhesitatingly and gratefully acknowledges his obligations for most of the facts and many of the quotations which will be found in this paper. Briefly stated things stand thus. There is a Bull of Benedict VIII, "De Sepulturis," issued in 1300, which deals with the cutting-up of dead bodies, and on which the whole of this accusation is based, and, so it would appear, based by those who cannot have taken the trouble to study the Bull itself.[1] The circumstances which called for the Bull were connected with the Crusades. It was the very natural wish of many Crusaders that, should they perish far from their own country, their bodies should be conveyed home in order

[1] The Latin of this Bull, with a full account of the whole question, will be found in Professor Walsh's work, *The Popes and Science*, an edition of which is published by the Catholic Truth Society, price 2s. 6d.

to be buried in their own family surroundings. Just imagine what the carrying out of such a request on behalf of a number of large-bodied warriors must have meant at that day! In most cases they probably could not embalm them; ships were small and usually overcrowded, and not to be had at any particular moment; in a word, not to prolong unpleasant considerations, there were a host of practical difficulties in the way of carrying out the requests of the dead Crusaders. Hence their survivors, casting about for some easy means of bringing something home for interment, adopted the horrible practice of cutting up the dead, boiling the flesh off the bones, and taking these, and these alone, home for burial. It was this practice that Boniface VIII forbad, and that fact is clearly proved by the opening lines of the Bull itself: "Corpora defunctorum exenternantes, et ea immaniter decoquentes, ut ossa a carnibus separata ferant sepelienda in terram suam, ipso facto sunt excommunicati." [1]

There is a rejoinder to this by those who have studied the Bull that, though it was originally meant to apply to the case of the Crusaders, it was afterwards and also applied to that of the study of human anatomy. Let us see how this statement fits in with the facts. One of the first things which a medical student learns is that there are scores of things in the human body which are known by the names of the men who described them first or who get the credit of first describing them. Some of these rather obscure persons would never have been heard of but for the fact that they have managed to get their names attached to some object—often a very obvious but sometimes a very trivial object—in the

[1] "Persons cutting up the bodies of the dead, barbarously boiling them, in order that the bones, being separated from the flesh, may be carried for burial into their own countries, are by the very act excommunicated.'

human body. Indeed, subsequent anatomists must often have envied the cheapness with which immortality could be purchased at the earlier stages of the history of their subject. If—as is not too often the case—the medical student further inquires as to who these persons were whose names have thus become known to him, he will not be long before he finds out that a very large number of them were Italians and flourished in Italy at a time when the Popes held sway over a considerable portion of that part of Europe and exercised a controlling influence at least over the rest of it. A still deeper probing into the facts will show that some of these, and by no means the least distinguished of them, were actually hardy enough to be Professors of Anatomy in the Papal City of Rome itself and fortunate enough to possess the friendship and patronage of the Popes themselves.

Two examples will suffice, and each shall be a man whose name at least is known to every medical student of the third year. Bartholomaeus Eustachius described the air-passage which leads from the back of the throat into the middle ear. Let the reader pause for a moment and perform the following perfectly safe evolutions : Take a deep breath ; shut the mouth ; hold the nose tight ; try hard to breathe out. The result will be that the drums of the ears will be felt to be deflected externally from the rush of air up the Eustachian tube, as it has been called ever since Bartholomaeus Eustachius, who died Professor of Medicine in Rome in 1574, first described it. It is perfectly clear that in order to describe it he must have first dissected it, and from the nature of the case probably dissected it many times, before he had found out all about it. At about the same period flourished Constantius Varolius, by whose name is still known a very important part of the brain called the Pons Varolii, which certainly could never

be seen by human eye without dissection. Goelicke, in his *Historia Anatomiae*, says of Varolius, "*Constantius Varolius*, Bononiensis, qui primum in Academia Patria professionem Chirurgicam habuit, deinde vero in aulam Romanam, Gregorio XIII, Pontificatum tenente, evocatus et Papae Archiater constitutus est, simulque in Archi lycaeo Romano Anatomiae professionem suscepit;"[1] and he then goes on to describe his connection with the Pons, as just mentioned, and to state that he first described the optic nerves. It is clear, then, that in the sixteenth century anatomy was going on in Rome under the favour of the Pope. But our opponents have not yet done, and their last ditch is a statement that between 1300 and 1550, two hundred and fifty years had rolled by—which we do not dispute—and that the Bull had lost effect in that time. In passing one may say that this scarcely fits in with "Rome's unbending, rigid, unchanging ways" as sternly reprehended by the same critics; but let that pass.

Neither does it fit in with the facts of the case. Mondino, who was an early writer on anatomy—one cannot call him the first writer, for the first book that we know of was on anatomy and its author was an Egyptian king—but a very early writer, was a Bolognese and published a dissecting manual which was in use for two centuries after his death in the early part of the fourteenth century. There is no kind of question that he did dissect some bodies. Our opponents, who cannot deny this fact, try to make out, from a statement which, it may be added, in no way bears out their contentions, that he only dissected three—still, if that were true, he did dissect three and had the audacity to publish the

[1] Constantius Varolius, of Bologna, first in the University of his home Professor of Surgery, was then called to Rome, Gregory XIII being Pope, and was made chief physician to the Pope and at the same time Professor of Anatomy in the Roman University.

results in the teeth of what it is urged were the provisions of the Bull. As a matter of fact, Guy de Chauliac, who studied anatomy in Bologna under Mondino's successor, Bertrucius, when he came to write his own work, *La Grande Chirurgie*, stated that Mondino had done many dissections—"et ipsam fecit multitoties."

So much for this accusation, which could never have been made by any one conversant with the Bull and with the history and practice of anatomy. Let us now turn to the remarkable story of Nicolaus Stensen, which presents so many surprising features that it reads more like a tale than a sober piece of reality.

Nicolaus Stensen was born in Copenhagen in 1638 on January 20th. His parents were Lutherans, as were the overwhelming majority of the inhabitants. His father was a goldsmith, but died when Nicolaus was young, when his mother married another of the same craft.

As the son grew up he received a wide general education, and became a student in the University of Copenhagen, where he turned his attention to medical studies. At that time, and indeed for many years, the anatomical chair at Copenhagen had been held by one or other member of the family of Bartholin, a name well known in anatomical text-books—one of them by a curious coincidence having his name attached to a salivary duct, as was the case with Stensen. Whilst a medical student, Stensen, as if it were designed that he should make experiments in all sorts of ways of living, actually had to become a soldier, for his city, being besieged by the Swedes, a regiment of students, known as "the black coats" from the colour of their clothes, was formed, and amongst the names of those composing it is to be found that of Stensen. After three years at Copenhagen, Stensen, according to the custom of those days,

migrated to Amsterdam, where one Blasius—who has somehow or other escaped having his name attached to anything known to me—was then anatomist. Whilst he was actually a student here Stensen discovered the duct of the parotid gland, which has ever since been called after him, "the duct of Stensen," or "ductus Stenonianus" from Steno, the Latin form of his name. The history of this matter may here be dealt with, though the full account of his services to anatomy will be reserved for a later part of this paper. The *Encyclopædia Britannica* (ed. xi), not very accurately as we think, says that Stensen "re-discovered" the duct of the parotid, though there is no statement which I have been able to find respecting the actual discoverer of that passage. The reader may now relax himself by another physiological experiment on his own body. Let him clench his teeth tightly and rub his fingers up and down over the central part of the large muscle which lies in front of his ear. He will feel a structure rolling under his fingers something like a piece of whipcord, and if he pursues the operation for a few seconds he will be conscious of a flow of saliva into his mouth. The structure which he has been rolling under his fingers is Stensen's duct, and the flow of saliva has come from the largest of the salivary glands —the parotid—which lies in front of the ear and in part on the muscle of which we have been speaking, the masseter.

As we have said, when Stensen made this discovery, which had been preceded by the discovery of the duct of the sub-maxillary, another salivary gland, by Wharton, an English anatomist, he was a student of Blasius, and Blasius, perhaps scenting immortality, made claim to be the real discoverer. It is possibly to this claim that the *Encyclopædia* is alluding, but it would seem to be perfectly untenable. First of all there is the fact

that Blasius in a book now long forgotten described the parotid as an organ whose function it was to keep the ear warm, which one cannot suppose that he would have done had he ever understood what the duct was. Secondly, there is the statement made by Bartholin of Copenhagen, who wound up the controversy by writing to Stensen his former pupil :—

"Your assiduity in investigating the secrets of the human body, as well as your fortunate discoveries, are highly praised by the learned of your country. The fatherland congratulates itself upon such a citizen, I upon such a pupil, through whose efforts anatomy makes daily progress, and our lymphatic vessels are traced out more and more. You divided honours with Wharton, since you have added to his internal duct an external one, and have thereby discovered the sources of the saliva concerning which many have hitherto dreamed much, but which no one has (permit the expression) pointed out with the finger. Continue, my Steno, to follow the path to immortal glory which true anatomy holds out to you."

This definite statement we may take it disposes of Blasius and establishes Stensen as the first discoverer of the duct named after him. From this period date those further and even more important anatomical discoveries of which mention will be made at a later part of this paper.

We have now to describe Stensen's sojourn in Italy and his conversion to the Catholic Church. With respect to this and the further movements of Stensen there is some discrepancy in the various accounts given of his career. For example, the *Encyclopædia Britannica* (ed. xi, *sub voce* Steno) says, "After a period of travel he settled in Italy (1666) first as Professor of Anatomy at Padua, and then in Florence as house physician to the Grand Duke Ferdinand II

of Tuscany. He returned to his native city in 1672 to become Professor of Anatomy, but having turned Roman Catholic, he found it expedient to return to Florence, and was ultimately made Apostolic Vicar of Lower Saxony." The whole article—a very short one—on Stensen is very inaccurate, and the slovenly statement, "having turned a Roman Catholic," &c., would certainly be taken by most readers to mean that, having become a Catholic in Copenhagen whilst occupying the Chair of Anatomy in that city, he found it expedient to leave. This in no way represents the facts of the case. Again, in the very excellent and laudatory account of Stensen's geological work given by Professor McKenny Hughes[1] there is the following statement, "In Paris he (*sc.* Stensen) became intimate with Thevenot, and here also he made the acquaintance of Bossuet. The eloquence and earnestness of that remarkable prelate had such an effect upon Steno that in 1667 he went over to the Catholics, which perhaps helped somewhat to secure for him the warm reception accorded to him by the Grand Duke Ferdinand II and his brother Leopold."

This again would seem to be an imperfect account of the facts. He settled in Florence in 1666, as the writer just quoted admits; as he also states, he did not become a Catholic until 1667. It is not quite clear, then, why the Grand Duke should have welcomed him as a Catholic some time before he joined the Church.

Professor Walsh gives an account which is evidently the result of careful research, which sets forth the facts in a simple and intelligible manner, and which we shall follow in this paper.[2] After the completion of his studies Stensen hoped to have been appointed Professor of Anatomy in Copenhagen, but was, for the time, disap-

[1] *Nature*, March 23, 1882.

[2] It will be found in his *Catholic Churchmen in Science.*

pointed, the appointment going to Jacobson, also a man of real distinction. Stensen left his home and went to Paris, where he worked for some years, and subsequently to Italy, which he reached in 1665. In the next year he is heard of in Rome, but shortly migrated to Florence, where he was made body physician (the term "house physician" used by the writer in the *Encyclopædia Britannica* is applicable to a young man in an English hospital, but not to the kind of position held by Stensen), and further was appointed physician to the Hospital of Santa Maria Nuova. As both of these appointments fell to his share before he became a Catholic a picture of toleration is brought before our eyes very different from that summoned up by those writers whose object it is to depict Catholic countries as unwilling to admit non-Catholics to any share in the good things which may be going. Stensen's conversion arose directly from his connection with the hospital just mentioned. The apothecary's department connected with the institution, which was necessarily frequently visited by Stensen, was under the charge of a nun, Sister Maria Flavia, who may be looked upon as a kind of head woman-dispenser.

Stensen may have been disposed to look favourably upon the claims of the Church from what he had seen and heard of Bossuet whilst in Paris, and from what we gather of his character from his writings there can be little doubt that his personality would be very attractive to those with whom he came in contact. Sister Maria Flavia, at any rate, made up her mind to make a convert of the distinguished young physician, and, as one might expect, inaugurated her campaign by constant prayers for that intention. She followed this up by inducing him to consider and even to participate in a number of Catholic practices, and finally she brought him into contact with a priest with

the result that, his difficulties having been cleared away, he was received into the Church. Sister Maria Flavia had been just about the same number of years in religion as Stensen had been alive when he became a convert, but she lived to receive the first blessing that he gave to any one after being consecrated a Bishop, and even wrote, at the direction of her confessor, a short account of Stensen's conversion.

It was after this event that Stensen received the offer of the Chair of Anatomy in Copenhagen. Seeing the important positions which he had secured in Florence, the Catholic surroundings and the beauty of the place, one may well wonder that Stensen should have accepted the invitation which he had so much desired at an earlier part of his career. It may have been, as has been suggested, that he hoped that he might be able to induce some of his fellow-countrymen to look with a less prejudiced eye upon the religion which he had come to love. At any rate, the offer was accepted and Stensen returned to the scenes of his student life. However, his stay there was of no very great length ; the toleration which he as a Protestant had experienced in Catholic Florence does not seem to have been extended to him as a Catholic in Protestant Copenhagen. One rather wonders that the invitation was ever given. In any case, Stensen resigned his position and returned to Italy, where various posts were at his disposal. None of these, however, attracted him, for he had made up his mind, in the height of his fame, to enter Holy Orders, which he did as soon as he had made the necessary preparations for the solemn ceremonies of ordination to the priesthood. From the statement of his contemporary, the Cardinal Archbishop of Florence, it is clear that Stensen was a man of great sanctity of life, for he says (as quoted by Professor Walsh, from whom, as I have said, most of my quota-

tions are taken), "Already as a member of a Protestant sect he had lived a life of innocence and had practised all the moral virtues. After his conversion he had marked out for himself so severe a method of life and had remained so true to it that in a very short time he reached a high degree of perfection."

Stensen's first years as a priest were spent in Italy, and during this time his great fame as a man of learning brought him in contact by correspondence or otherwise with many of the distinguished men of the day, Spinoza and Leibnitz being two outstanding examples. It was during this time also that he devoted himself to those studies in geology which form the foundation of all modern ideas in connection with that science and rendered him, even in his own day, as illustrious a man in that branch of knowledge as he had previously made himself in anatomy.

All this peaceful life of study was, however, to come to an end; for, at the request of the Duke of Hanover, most reluctantly—indeed only after he had been put under holy obedience—he was consecrated a bishop for that part of Germany. As has already been told, the first action which he performed after his consecration, was to write to Sister Maria Flavia and send her his benediction, she having, under Almighty God, been the instrument of his conversion. Every convert knows that there is a certain type of mind which cannot credit an honest conversion—an awful self-revelation!—and feels quite sure that money or the hope of a beautiful bride or some other mundane temptation has been the inducement to a step which, so it would appear, no man in his senses would otherwise have taken. Every one of us who is a convert and outside the workhouse knows very well this kind of thing, and one feels convinced that Stensen had many wagging fingers pointed at him when he was raised to the episcopal dignity.

"A fat bishopric," that is the elegant way in which certain lewd journals of the baser sort would put it nowadays. Well, Stensen made little of temporal profit from his new dignity. He utterly refused the carriage which the Duke wished to provide for him ; he would not even have servants, in the ordinary sense of the word, in his house, for the few persons who lived with him and shared the household duties were converts with whom he lived on terms of perfect equality. And when this occupant of two "fat bishoprics" came to die there was not even enough money forthcoming to pay for his burial. All that he had, besides his clothes, were his episcopal cross and ring and a few relics of SS. Ignatius Loyola, Francis Xavier, and Philip Neri, which he held in great veneration.

There is another idea entertained about most—probably all—converts, as again we who have been through the mill know, and that is the idea that they are very unhappy where they are, and are either on the point of coming back to the fair fields of Protestantism from the dark prison of Popery or would do so if their pride would let them confess what a terrible mistake they had made. These kindly remarks—for, unlike the first-named suggestions, they are often meant to be quite kindly and are made by those in whose hearts is a real friendship for the unfortunate convert—may well have been made concerning Stensen, and it may be well to give his own account of his feelings towards the religion which he had embraced in the full vigour of his manhood. Writing to a friend he says, "To-morrow I shall finish, God willing, the eighteenth year of my happy life as a member of the Church. I wish to acknowledge once more my thankfulness for the part which you took under God in my conversion. As I hope to have the grace to be thankful to Him for ever, so I sigh for the opportunity to express my thankfulness

to you and your family. I can feel that my own ingratitude toward God, my slowness in His service, make me unworthy of His graces; but I hope that you who have helped me to enter His service will not cease to pray, so that I may obtain pardon for the past and grace for the future, in order in some measure to repay all the favours that have been conferred on me."

Hanover and Hamburg, where Stensen lived for a time, did not afford, as he thought, much chance for missionary labours, and what could be done was being admirably done by the Fathers of the Society of Jesus. Hence when the Duke of Mecklenburg-Schwerin became a Catholic and asked that Stensen might be sent to him as a Bishop the request was complied with, and in hard missionary work in that part of Germany Stensen spent the remainder of his life. It is sad to have to say it, but his labours seem to have borne but little fruit according to the judgement of this world, at any rate in conversions. In this apparent waste of a life —one must emphasize the word "apparent," for who but Almighty God can say whether there was waste or was not?—there is a certain parallel between Stensen and another great Catholic man of science—Mendel. Mendel was torn away from his monumental work to become Abbot of Brunn, and his labours as a scientific inquirer came to an end. At what reward? Apparently at none, for the rest of his life is a tale of struggle for the rights of his Abbey, a struggle for which a less valuable life might have equally well sufficed. And Stensen was cut off from his never finished geological task to spend a life of labour with, as we have said, but little apparent fruit.

What both of them did, however, was to present a splendid example of sense of duty over-riding evident desire and obvious fitness for another and totally different line of life. After Stensen's death the Medici

family, remembering the man who had once been an ornament of their city and had there received the gift of Faith, begged his body and had it interred in San Lorenzo in Florence. The tablet then set up is more concerned, as will be seen, with his religious than with his scientific history; it was left for later comers in a distant century to set up a second inscription (both are given at the end of this paper) commemorating his great position in the world of science. To the works which gained him this position we must now turn our attention. They divide themselves quite simply and naturally into two groups both by subject and by time. There are the earlier group of anatomical observations made partly in the north and partly in the south of Europe, and all, or nearly all, of them in his Lutheran days. Then there is the second group of geological writings carried out in Italy and after he had entered the Catholic Church.

Amongst the anatomical group attention has already been drawn to his discovery of the duct of the parotid gland, because it is on account of this duct that his name is chiefly known and, it would seem, will always be known. But it is not his most important contribution to science, not even to anatomical science. The contribution, so far as that group goes, of really prime importance was his discovery that the heart is a muscle. "The heart a muscle! What else could it be?" will be the exclamation which will rise to the lips of many when they read this statement. It is easy so to think now, but let us just picture how things were when Stensen made his discovery. Histology, or the science of the tissues, was a field of knowledge quite unexplored; physiology, in any proper sense of the word, almost equally unknown; even the fact of the circulation of the blood was only slowly making its way into the minds of men. Harvey, one of England's greatest

men, Harvey who—how difficult to imagine it!—was actually tutor to Charles II and James II when boys, and was engaged in teaching them in a neighbouring field whilst the Battle of Edgehill was being fought—Harvey was the man who first proclaimed the fact that the blood circulated throughout the body. But Harvey's views were looked upon as a mere chimæra by many, and it required two further discoveries to establish them. Arteries and veins, the passages by which blood comes from or returns to the heart, are easy of demonstration and can be seen by anybody, but how does the blood get from the arteries to the veins? Everybody nowadays knows that this takes place through a series of fine tubes, invisible save to the microscope, which are called capillaries. Everybody knows it now, but nobody knew it until Malpighi, a contemporary of Stensen's, made the discovery. Then, in the next place, what is it that drives the blood to rush upon its constantly recurring course? Every one now knows that it is the contraction of that great muscle, the heart, but nobody knew it until Stensen proclaimed the fact and with it gave the key to the motive power of the circulation. This was not the only piece of work in myology, or the science of muscles, in which Stensen engaged, for, in fact, he wrote largely on this branch of anatomy. Further he gave accurate and original descriptions of the blood-vessels of the nose and also of the lachrymal gland, the gland which produces tears, and its passages. If Stensen had never done anything else, in anatomy he had earned for himself a place amongst the immortals. But he was to add a far greater glory to this. In anatomy he had been a member—a very brilliant member, but still only one member—of a band of discoverers who were then creating a solid science, but in geology he was to be a pioneer and the father of his science, the author of what

the Germans aptly call a "bahnbrechenden Werke," the layer of the foundation on which all subsequent geological work stands. That these high-sounding words are not mere empty praise, the outcome of religious partisanship, must now be proved.

First of all, we must try to understand the state of opinion when Stensen began his labours. Take the case of fossils. Of course their existence had been recognised; fossil sharks' teeth were found in quantities in Italian deposits, and especially in Malta, where tradition alleged that they were the teeth of the snakes which were deprived of them by St. Paul. But as to the nature of fossils there was no proper explanation. One school of writers believed that these objects had been created just as they were found, they were "lapides sui generis," things which never had been different from that which they now were, freaks or superfluities of creation. Another group, coming nearer to a recognition of their real character, believed that they had originally been portions of living things, but that their position and condition were due to the terrific catastrophe of the Noachian deluge, which was regarded also as responsible for many features of physical geology. Still another group conceived that the fossils, originally belonging to living creatures, had been buried by eruptions from volcanoes. Of course, the rocks in which they are found are never volcanic, but one must remember that petrology was hardly in its infancy at this time. Now Stensen had dissected and published an account of a shark captured off Leghorn in 1667, and had paid especial attention to the nature of its teeth, so that when his attention was directed to the fossil examples of a similar species he was in a position to grasp their meaning readily. It was then that he set himself to write a treatise for the Della Cruscan Academy, of which unfortunately

he never achieved more than the introduction, in which he intended to lay down his views on geology. This introduction is entitled "De solido intra solidum naturaliter contento Dissertationis Prodromus." It was published in quarto at Florence in 1669, and is not merely an introduction to the work which was never destined to appear, but is the introduction to all the many volumes which have since been written on geology.

Dealing first of all with the question of fossils, as that has already been alluded to, Stensen lays down this basic law, applicable not only to them, but to other geological facts, that "if a given body of definite form, produced according to the laws of nature, be carefully examined, it will show in itself the place and manner of its origin." Now, applying this to the case of fossils, Stensen showed first of all that these were the remains of living things, claiming that even if sea-shells were not known to exist nowadays, yet one must believe that the fossil examples had belonged to living creatures. Then he showed that whilst some of the fossil shells were found just as they had been left, others had suffered a replacement of their structure, whilst others again were only represented by casts of what had been shells. Finally, as to their position, he argued that one must think of the shells and their surroundings and argue from them to the manner of their deposition just as one would do about any other collocation of objects. For example, he says that if we found a collection of sea-salt, planks of ships, and marine animals we should argue that the sea had been there, even if we were unable to decide whether the reason that it was not there now was because the land had been raised or the sea had been lowered. And, again, he says that if we find charred pieces of wood and other objects of the same kind we argue that there has

been a fire. It is obvious to any one who considers the matter that in this argument he lays the foundation of all later geology, and of the greater portion of prehistoric archæology also. But his discourse is not confined to the subject of fossils. He deals, and for the first time scientifically, with the subject of stratification. The examination of the natural object with the idea of discovering how it was formed led him to formulate the theory of the deposition of aqueous rocks. "The powdery layers of the earth's surface," he says, "must necessarily at some time have been held in suspension in water, from which they were precipitated by their own weight. The movement of the fluid scattered the precipitate here and there, and gave to it a level surface." And again, "Bodies of considerable circumference which are found in the various layers of the earth, followed the laws of gravity as regards their position and their relations to one another. The powdery material of the earth's strata took on so completely the form of the bodies which it surrounded that even the smallest apertures became filled up, and the powdery layer fitted accurately to the surface of the object, and even took something of its polish."

If they were laid down in water such strata must necessarily have originally been horizontal. Stensen recognized this fact, and of course also recognised that, so far from being horizontal, many aqueous rocks are tilted, contorted, and otherwise altered from their original position, and this he accounted for, much as we account for it now, by disruptions through volcanic effort and by the collapsing of cavernous spaces underground. Such changes of the contour of the earth naturally led him to consider the question of mountains. "All the mountains which we see now," he says, "have not existed since the beginning of things." Yet, on the other hand, mountains do not grow like

plants, nor do they all run in certain directions, as some had claimed. Pursuing this subject, he shows that mountains with flat tops are made up both of horizontal and inclined strata, and that regions where mountains exist are raised and depressed, and are subject to rending and fissuring. From this he passes to the question of springs, and shows that one of the effects of the dislocation of strata to which he called attention was the opening of fissures through which internal collections of water could escape. He endeavoured to reduce the strata of Tuscany to six periods, and thought that these might be demonstrated also in other parts of the world. Further, he fully recognized and was not afraid to claim that immense ages must be granted for the carrying out of the processes which he postulated.

Any one who is acquainted even superficially with modern geology will see how closely these views resemble many of its teachings. What they have to remember is that these views were now formulated for the first time, and that, if they remained comparatively unknown for some time, that must in large measure be due to the fact that Stensen "left his tale half-told." There is another resemblance between Mendel and himself in the fact that Mendel's epoch-making discoveries slumbered for years in the pages of an obscure publication till rediscovered in our own days. If Stensen's theories took their time in conquering the world, they did conquer it ; for every writer on geology has conceded to him the honour which is so justly his due.

In a previous portion of this paper the fact has been mentioned that the Medici placed over his tomb an inscription commemorative rather of his piety than of his scientific fame. In 1881 the International Congress of Geologists met in Bologna and, after the sessions of their body had come to an end, the members paid a visit

to Florence in order to see the tomb of one to whom their science owed so much. They also placed there a tablet on which, quite naturally, they insist upon his fame as a man of science. The inscription and its translation appear at the end of this paper. Here surely is a striking object lesson as to the possible relations between Religion and Science. Stensen was a man of indubitable piety : that may be gathered from what has gone before. He forsook everything to follow his Master, and what a sacrifice that must have been none can realize but those who have known the fascination of scientific research and the pang that pierces the heart when cut off from it. He lived a life of apostolic poverty and mortification. He advanced far-reaching theories as to geology, widely differing from those held at that day, and straining almost—as it would then seem—the beliefs of the orthodox. Some writers of the present day say that he advanced these views with caution because he was afraid of the Church and her terrors. It is not too much to say that if he had not been a Catholic these same writers would have lauded to the skies his scientific caution in not going one step beyond what he believed he could completely prove. The fact remains that he did advance these views without any reserve, and that the only torture which the Church inflicted upon him—no doubt it was a heavy burden to be borne—was that of making him a bishop and sending him out to work "in partibus infidelium," and, as one may well say, "in terra deserta et inaquosa."

The visitor to the University of Glasgow, an institution which will not feel it an insult to be described as a centre of ardent Protestantism, cannot but feel a shock of surprise when he enters the Senate Room and finds that the central object of interest over the fireplace is a well-executed relief of a Pope crowned with the tiara.

Thus the University does honour to its founder, Nicholas V, who by his Bull—under the authority of which the University still exists—in 1450 first established that celebrated seat of learning. In like manner at the University of Copenhagen amongst the portraits of dead and gone celebrities—some now long forgotten—the visitor may experience the same kind of shock in coming across the portrait of a Catholic bishop. His delicate, thoughtful, rather sad face is surmounted by the biretta, and his right hand, with its episcopal ring, fingers the pectoral cross which hangs upon his breast. It is the portrait of Bishop Stensen, one of the greatest of the glories of the Academy of which he was once a student and for a short time a Professor ; a man who walked with God and studied His creation whilst on earth, who departed this life in the odour of sanctity, and who has—can one doubt it ?—by now received the reward of the man who forsakes father and mother and all things earthly to follow the light which shines upon his path.

THE TABLETS ALLUDED TO ON P. 16.

A. *The Original Tablet.*

Nicolai Stenonis Episcopi Titopolitani Viri Deo Pleni Quidquid Mortale Fuit Hic Situm Est. Dania Genuit Heterodoxum Etruria Orthodoxum Roma Virtute Probatum Sacris Infulis Insignavit Saxonia Inferior Fortem Evangelii Assertorem Agnovit Demum Diuturnis Pro Christo Laboribus Aerumnisque Confectum Sverium Desideravit Ecclesia deflevit Florentia Sibi Restitui Saltem In Cineribus Voluit. A.D. 1687.

Whatever was mortal of Nicholas Stensen, Bishop of Titopolis, a man full of God, lies here. Denmark brought him forth a heretic, Tuscany a convert, Rome, his virtue being proved, decorated him with the sacred mitre, Lower Saxony recognized a brave preacher of the gospel. Then, worn out by daily labours for Christ and by tribulations, Schwerin lamented him. The Church sorrowed for him. Florence desired that at least his ashes should be restored to her. A.D. 1687

B. *The Tablet erected in* 1881.

Nicolae Stenonsis Imaginem Vides Hospes Quam Aere Collato Docti Amplius Mille Ex Universo Terrarum Orbe Insculpendam Curarunt In Memoriam Ejus Diei iv Cal Octobr. An. MDCCCLXXXI Quo Geologi Post Conventum Bononiae Habitum Praeside Joannio Capellinio Equite Huc Peregrinanti Sunt Atque Adstantibus Legatis Flor Municipii et R. Instituti Altiorum Doctrinarum Cineres Viri Inter Geologos Et Anatomicos Praestantissimi In Hujus Templi Hypogaeo Laurea Corona Honoris Gratique Animi Ergo honestaverunt.

You behold here, traveller, the bust of Nicolas Stensen, as it was set up by more than a thousand scientific men drawn from the whole world, as a memorial to him on the fourth of the Kalends of October 1881. The geologists of the world, after their meeting at Bologna, under the presidency of Count John Capellini, made a pilgrimage to his tomb, and in the presence of the chosen representatives of the municipality, and of the learned professors of the University, honoured the mortal ashes of this man, most illustrious amongst both geologists and anatomists.

ALOISIO (LUIGI) GALVANI

ALOISIO (LUIGI) GALVANI (1737–1798)

AND SOME OTHER CATHOLIC ELECTRICIANS

BY

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To become a byword is not usually regarded as a very creditable performance, and the man who incurs that fate is not generally thought to be a fit subject for congratulations. No one, for example, would crave the position of originating the verb " to burke," and no one also would care to endure the unpleasantnesses which fell to the lot of its god-father in order to go down to history as the cause of the addition of the word " boycott " to the English language.

But as there are inglorious ways of becoming a byword, so also there are glorious methods. To have one's name attached for ever by brothers in science to the name of some epoch-making discovery is certainly no discreditable method of ensuring immortality for one's reputation. In the great field of physical science many men's names will be found attached to " laws " or discoveries of various kinds, and it is of some of these that this paper proposes to treat, pointing out at the same time the remarkable fact that a group of the most distinguished of them were almost all Catholics, and that those who were not were men of undoubted piety and of firm attachment to Christian principles. This statement is

certainly opposed to the idea which has been so often put forward of recent years, that Science and Christianity are opposed to one another, but it remains, nevertheless, a fact, as will be readily seen from what is to follow. Galvani's name has been chosen as the chief example, not because he was the greatest of all the band of electricians of whom we have to speak, but because the words derived from his name are probably in more general use than those which have originated from the names of the other discoverers, on whose achievements we shall briefly touch.

"Galvanism" first came into the world as a kind of alternative word for what we more commonly call electricity, and was the first derivation from the name Galvani. Then manufacturers took up the matter and discovered the plan of coating one metal with another by electrical methods, and thus arose the verb "to galvanize." Finally,—and this shows the complete incorporation of the word into the language—it even begins to take on a derived or metaphorical meaning, and we hear of a body of indignant constituents "galvanizing into activity" their slothful representative in Parliament. As has been already said, Galvani, the consideration of whose life-history we defer for a short time, was only one of the discoverers dignified by science with terminological honours.

When we read of a "voltaic battery" or, as we so commonly do in these days of electrical power, of a "volt," we are doing homage to the memory of one of the greatest, if not the greatest of all the electrical pioneers, **Alessandro Volta** (1745–1827), of whose discovery of the electrical "pile" so great a man of science as Arago says that it is "the most wonderful instrument that has ever come from the hand of man, not excluding even the telescope or the steam-engine." Now, in addition to his greatness as an electrical discoverer, as to which it is neither possible nor indeed necessary to say

more here, Volta was a most devout and convinced Catholic. Towards the end of his career he retired to a country house near Como, and, as Professor Walsh says, whilst living there his piety " became a sort of proverb among the country people. Every morning at an early hour, in company with his servant, he could be seen, with bowed head, making his way to the church. Here he heard Mass, and usually the office of the day, in which all the canons of the cathedral took part. He had a special place on the Epistle side of the altar, not far from the organ. His favourite method of prayer was the rosary." His own confession of faith, which he drew up and signed in 1815, that is to say, twelve years before he died, is conclusive. This confession was drawn up by himself and without suggestion from anybody, because some tattlers had spread the story that he attended to his religion lest he should hurt the feelings of some of his friends. We quote it at length, as it is not only of singular interest in showing the kind of man Volta was, but is also remarkably applicable to the present day. This is what he wrote :—

" If some of my faults and negligences may have by chance given occasion to someone to suspect me of infidelity, I am ready, as some reparation for this and for any other good purpose, to declare to such a one and to every other person and on every occasion and under all circumstances that I have always held, and hold now, the Holy Catholic Religion as the only true and infallible one, thanking without end the good God for having gifted me with such a faith, in which I firmly propose to live and die, in the lively hope of attaining eternal life. I recognize my faith as a gift of God, a supernatural faith. I have not, on this account, however, neglected to use all human means that could confirm me more and more in it, and that might drive away any doubt which could arise to tempt me in

matters of faith. I have studied my faith with attention as to its foundations, reading for this purpose books of apologetics as well as those written with a contrary purpose, and trying to appreciate the arguments *pro* and *contra*. I have tried to realize from what sources spring the strongest arguments which render faith most credible to natural reason, and such as cannot fail to make every well-balanced mind which has not been perverted by vice or passion embrace and love it. May this protest of mine, which I have deliberately drawn up and which I leave to posterity, subscribed with my own hand, and which shows to all and everyone that I do not blush at the Gospel—may it, as I have said, produce some good fruit.

"Signed at Milan,

"January 6, 1815,

"ALESSANDRO VOLTA."

Volta, as we shall see, was a contemporary of Galvani, and closely associated with him in some of the discoveries which made their names memorable—a greater discoverer according to some, and the man after whom the Unit of Electro-Motive Force is named. In these days we constantly read of a current of such and such a "voltage," or learn that along the wires which actuate our tram service runs a current of so many "volts." It is not likely that this terminology, which has now become universal, will ever be disturbed, and as long as it lasts, so long will the name of Alessandro Volta be constantly brought before the minds of men. In another part of this paper further facts concerning Volta will be given in connection with his famous controversy with Galvani.

A second Unit of which the general public hears less, but which is in constant employment amongst electricians, is the Ampère, which is the Unit of Current. It commemorates the name of **André Marie Ampère** (1775–

1836), Professor of Mathematics in the École Polytechnique, who, it may be pointed out, lived through the troublous time of the French Revolution, yet carried his firm Catholic faith to the end. It is related of him that, after his conversations with Ozanam on the problems of science and philosophy, he was in the habit of exclaiming, " How great is God, Ozanam! How great is God and how little is our knowledge!" And the same Ozanam's testimony to his religious fervour may here be set down. " In addition to his scientific achievements, this brilliant genius has other claims upon our admiration and affection. He was our brother in the faith. It was religion which guided the labours of his mind and illuminated his contemplations: he judged all things, science itself, by the exalted standard of religion. . . . This venerable head, which was crowned by achievements and honours, bowed without reserve before the mysteries of faith, down even below the line which the Church has marked for us. He prayed before the same altars before which Descartes and Pascal had knelt; beside the poor widow and the small child who may have been less humble than he was. Nobody observed the regulations of the Church more conscientiously, regulations which are so hard on nature and yet so sweet in the habit. Above all things, however, it is beautiful to see what sublime things Christianity wrought in his great soul: this admirable simplicity, the unassumingness of a mind that recognized everything except its own genius; this high rectitude in matters of science, now so rare, seeking nothing but the truth and never rewards and distinctions; the pleasant and ungrudging amiability; and lastly, the kindness with which he met everyone, especially young people. I can say that those who know only the intelligence of the man, know only the less perfect part. If he thought much, he loved more."

The third Unit, that of Quantity, is called the Coulomb,

and owes its name to **Charles Augustin de Coulomb** (1736–1806), also a Catholic, of whom his biographer Biot says: "Coulomb lived among the men of his time in patience and charity. He was distinguished among them mainly by his separation from their passions and their errors, and he always maintained himself calm, firm, and dignified *in se totus, teres atque rotundus*, as Horace says, a complete, perfect, and well-rounded character."

The fourth Unit is that of Resistance, and it is called the Ohm. **George Simon Ohm** (1789–1854), after whom it is named, was Professor at Nuremberg and subsequently, and until the time of his death, at Munich, where he is buried. There is some doubt as to the religion which he professed, though it is probable that he was a Catholic. We will not, however, claim him as such, and will content ourselves by pointing out that his first teaching appointment of any importance was at the Jesuit Gymnasium at Cologne, the principal place of instruction for Catholic youth of the Rhineland; and that he held the position of Professor of Mathematics in that institution for ten years. If not the rose, he spent a long time very near the rose. But we incline to the belief that he was a Catholic.

As to the person after whom the fifth Unit, that of Capacity, is named, there is no doubt. The Farad is called after **Michael Faraday**, who certainly was not a Catholic, but a member of a small Christian sect. He was, however, as his life testifies, a man fully convinced of the truth of the Christian doctrines.

All the six persons whom we have named were adherents of Christianity, and four of them, at least, were Catholics, a fact which will take a good deal of explaining away on the part of those who are never tired of urging the irreconcilable claims of Religion and of Science. It is now time to turn to the history of Galvani, whose name is set at the head of this paper,

and of his celebrated controversy with Volta, an historical episode in the tale of Experimental Science.

Aloisio (Luigi) Galvani, one of the greatest scientists of the eighteenth century, was born in Bologna on the 17th of September 1737. In early life he, like Johannes Müller, and indeed more than one other man who afterwards became great in the roll of science, wished to enter the Church, but later decided to embrace a medical career. He obtained his medical degree in 1762. His graduation thesis on the human skeleton, dealing mainly with the formation and development of bone, gained for its author the position of Lecturer in Anatomy in the University of Bologna. Although not a fluent speaker, he was most popular as a teacher, and one of the first to illustrate his lectures by experiment. In 1775 he was promoted to the Chair of Anatomy. Galvani did much research work in comparative anatomy, and amongst other things wrote a rather remarkable treatise on the semi-circular canals of birds, in which, for the first time, he gave exact measurements of these curious structures, and pointed out the striking fact that in the hawk these objects are of singular size, being absolutely and not relatively larger than in any other bird, in man, or even in the horse or the cow. He did not, however, seize the significance of these canals, which we now believe to be connected with direction and equilibration. He wrote also a treatise on the kidneys of birds, which attracted a good deal of attention in the scientific world of the Europe of the day.

But it is with his discoveries in electrical science that his fame is, and always will be, associated. At the time at which he lived, the sciences of Anatomy and Physiology, that is of Structure and Function, were not separated from one another to the extent which they now are. It is, indeed, only within our own day, so to speak, that separate Chairs of Physiology from those

of Anatomy have been established in all Schools of Medicine and other seats of learning. Galvani was a Physiologist as well as an Anatomist, and in the former capacity he made a special study of the nervous system.

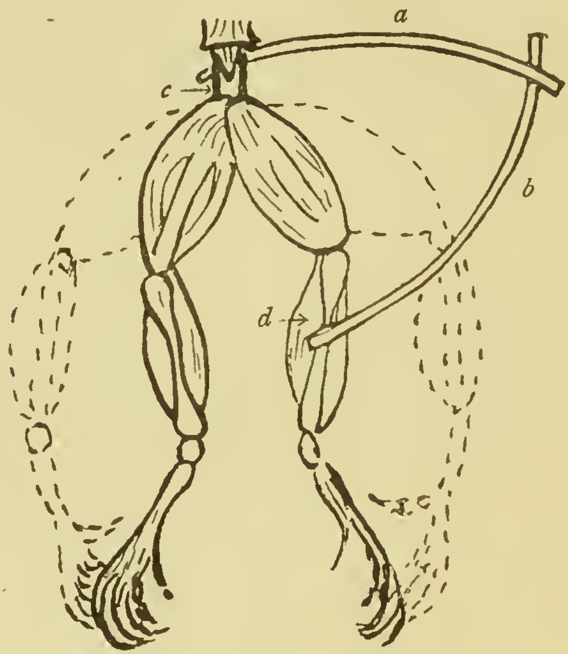
The susceptibility of the nerves to irritation was indeed a subject to which he had devoted many years of study. While engaged in this work one day in 1780 he tells us, "I dissected and prepared[1] a frog (see diagram and description), and laid it on a table on which there stood an electric machaine at a considerable distance from the frog. As one of my assistants accidentally touched with a scalpel the inner crural nerves of the frog, the muscles of the limbs were violently convulsed. A person who was accustomed to help us in our electrical experiments thought he observed that at the same moment a spark was drawn from the electric machine. Marvelling at this new phenomenon he called my attention to it, though at the time I was deeply engaged in other matters. I was suddenly inflamed with a great desire to try the experiment myself, and to bring to light what was hidden therein. I, too, touched the crural nerve with the point of the scalpel, and got someone at the same time to draw a spark from the electric machine. The same phenomenon as before occurred. Every time the muscles of the limbs were violently convulsed as if the frog were seized with tetanus at the instant the spark was drawn from the electric machine." Galvani's next experiment was to ascertain the effect of lightning. For this purpose he employed a long insulated iron wire reaching from his laboratory to nearly the top of the house where the wire projected into the open air. Its lower end was joined to the crural nerves of a freshly prepared frog, its legs being

[1] To prepare a frog, the animal is first killed, the hind legs are then detached and skinned, the crural nerves and their attachments to the lumbar vertebræ remaining. See fig., p. 10.

attached to another iron wire which was connected to the water in a well. Every time a lightning flash occurred the muscles of the frog exhibited convulsions. The insulated wire employed in this experiment was not without danger. Riechmann was killed at St Petersburg in 1753 by a discharge during a thunderstorm from a similar apparatus, a fact which must have been known to Galvani.

The susceptibility of the muscles of the frog to atmospheric electricity in calm weather next engaged Galvani's attention. His interest in the matter arose from his having observed " that prepared frogs suspended by brass hooks through the spinal marrow from an iron lattice round a hanging garden of our house, exhibited convulsions not only during thunderstorms but occasionally also in fair weather." At first, Galvani attributed these to variations in the electrical state of the atmosphere, but they were probably caused by contact of the iron lattice with the muscles of the leg. With a view to testing the matter further he carefully watched, during many days of calm weather, frogs suspended as above from the iron lattice. Much to his disappointment, the convulsions he looked for rarely occurred. At length, weary with waiting, he brought into a closed room a frog with a brass hook through its spinal marrow, and placing it on an iron plate he tells us, " when I pressed the brass hook, fixed in the spinal marrow, against the iron plate, behold ! the same contractions, the same movements as before. I tried other metals with the same result, except that the amount of contraction depended on the metals used." This is by far the most important discovery made by Galvani, in which he showed that when the nerves and muscles of a frog are joined by a metallic arc, generally formed of two metals, convulsions occur. These he ascribed to a fluid the same as electricity, which flowed from the nerves to the muscles through the connecting metallic

conductor. The accompanying diagram illustrates the way in which the experiment is usually performed—(*a*) and (*b*) denote two dissimilar metals, zinc and copper, for example, soldered together, (*c*) the crural nerves, (*d*) the muscles of the leg. At the instant the nerves and muscles are connected, as in the figure, by the metallic arc, contractions of the muscles occur, provided the experiment be made shortly after the animal has



been killed: a few hours after death the limbs lose this contracting power.

The publication of Galvani's experiments excited great interest in the scientific world, but his theory of animal electricity did not meet with general acceptance. Contemporary opinion appears to have rather inclined towards Volta, but Galvani's views are perhaps more in harmony with modern theories.

Galvani regarded the muscle of the frog as the seat of electrification, opposite electricities being stored in it, as in a Leyden jar; the nerves acting simply as con-

ductors. "That opposite electricities may be accumulated in one and the same muscle anyone will admit who has observed that the muscle fibres, although apparently very simple, are really built up of different solid and fluid parts which in no uncertain way point to a difference of substance." Alessandro Volta, in a letter written to Baronio in 1792, expresses the greatest admiration for "the astonishing discoveries of Signor Galvani." He began by accepting the theory of animal electricity, which he was led ultimately to reject in the course of repeating and varying Galvani's experiments. He ascribed the contractions of the muscle to the stimulating action on the nerves of electric currents, which he held were due, not to electricity inherent in the animal, but to the contact of the dissimilar metals which constituted the connecting arc. "The electricity acts on the nerves and on the nerves directly, however it may be produced; it is unnecessary to send the current from the nerve to the muscle; if it flow through a short length of the nerve, contraction of the muscle follows: the current is not the immediate cause of the contraction, but the remote cause in so far as it stimulates the nerve."

In support of his views, Volta describes the following interesting experiment in a letter to Aldini, a nephew of Galvani. "If, after the exposure of the crural nerves of a frog or ischiaticus of a sheep or other animal, I touch and press the nerve with the edge of a silver or gold plate, a coin, for instance, I see that nothing happens (sometimes a contraction occurs immediately after the nerve has been exposed, as it is then very sensitive and responds to the slightest touch); I then touch and press it with the edge of a zinc plate; and again I observe no change; finally, I touch it with the edges of both plates: immediately violent contractions take place of the muscle of the leg." At the time Volta made this experiment he was unaware that twenty-five years

earlier a similar result had been obtained by Johann Georg Sulzer, who noticed that if the tip of the tongue be touched by two plates, one of lead, the other of silver, and if at the same time the edges of the plates be brought in contact, an acid taste "similar to that of vitriol of iron," is produced, although no such taste arises when the tongue is touched by either of these metals alone. Volta first heard of Sulzer's observation from Aldini, and regarded it as confirmatory of his own views. "It is clear," he writes, "that in all these experiments the nerves only were excited by the electric current: it is clear that the metals were the cause of the current: they were the excitant and motive power of the electricity, the nerves being merely passive."

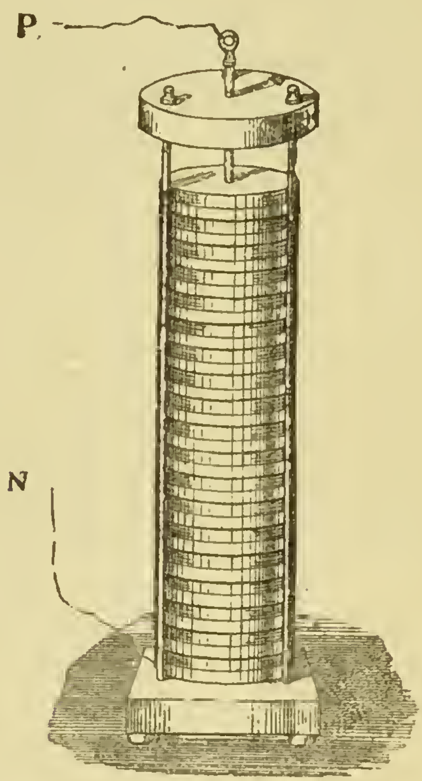
In the course of a long discussion between Galvani and Volta, and their respective partizans, Galvani, assisted by his nephew, Aldini, showed that contraction might be obtained either (1) by using one metal only, or (2) without the use of any metal. Cutting the nerves from the vertebral canal and delicately raising them by means of an insulating rod and placing them so as to touch at a single point the muscle of the thigh of a frog, contraction of the thigh immediately occurred. Galvani also succeeded in producing contraction by connecting the nerve to a muscular fragment of the belly, which lay apart on a glass plate and without any conducting connection with the frog. These experiments were held by Galvani's followers completely to disprove Volta's views. It is easy to see in his letters how much the latter was wounded by the arrogant tone in which the Galvinists, old and young, boasted of having reduced him to silence. The silence, however, was not of long duration. He pointed out that the success of these experiments depended on the employment of organs of the animal as different as possible, and on their being connected by a third substance. He extended his theory: holding that any two dissimilar substances,

whatever their nature, developed electricity by their simple contact.

In the space at my disposal I cannot enter more fully into this controversy. It was practically ended by the following experiment due to Volta. He placed in contact two plates, one of copper, the other of zinc, which were held by insulating (glass, for instance) handles. On rapidly separating the plates he showed, by means of an electroscope, that both plates were charged with electricity, the zinc positively, the copper negatively. By repeatedly connecting and separating the plates and communicating their charges to a condenser, he succeeded in obtaining an electric spark. This discovery that electricity is produced by the mere contact of dissimilar metals is one of the greatest achievements of physical science. Volta arranged the metals in a series such that any of them, touched by a metal below it in the series, became positively charged, and negatively if touched by one above it. For instance, iron is positive with respect to copper and negative with respect to zinc. The electric condition of two metals in immediate contact is the same as when one or more metals is placed between them. It follows from this that a closed metallic circuit, however many metals it embraces, does not produce an electric current, at least, so long as all the metal junctions are at the same temperature. If, however, two plates, of copper and zinc, for instance, be separated by a fluid, the difference of their electric condition, or potential, as it is now called, is not the same as when the two plates are in direct contact.

In 1800, a little more than a year after Galvani's death, Volta invented his marvellous electric pile. In a letter to Sir Joseph Banks he writes: "The apparatus, which will no doubt astonish you, consists only in the arrangement of a number of good conductors which follow one another in a regular order. Thirty, forty, or sixty discs of copper, or better, silver, on each of which

a disc of tin, or better, zinc, is placed, and each pair separated from the pair above it by a disc of moistened pasteboard (so that the order is copper, zinc, pasteboard, copper, zinc, pasteboard, and so on); such an orderly arrangement of the three kinds of conductors is all my apparatus consists of. It is capable of giving shocks (when the ends are simultaneously touched with moistened

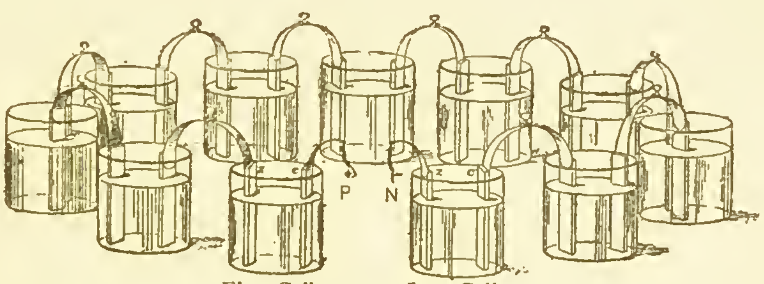


fingers), like that from a feebly charged Leyden jar of enormous capacity. . . . It is ever active without the aid of electricity supplied by any hitherto known means."

In the same letter Volta describes how, without altering the order of the conductors, he changed the shape of his apparatus into a form which is practically the same as that of the modern electric battery. "One takes a

row or ring of glass cups, half filled with water, or better, brine, and joined by the same number of metallic arcs; one arm of each arc is of copper, and dips into a cup, whilst the other arm, which is of zinc and soldered to the copper, dips into the next cup, and so on." When arranged in ring form, the battery was known as the *couronne de tasses.*

Volta attributed the action of his pile to the contact of dissimilar metals. In this he was certainly wrong. The intervening liquid conductor played a most important part: the electric current was maintained by the chemical action of this liquid conductor on one of the metals, on the zinc, in the zinc and copper pile.



First Cell. Last Cell.
P is extremity of circuit on the left. N is extremity of circuit on the right.

That chemical action was the cause of the currents obtained by both Volta and Galvani was suggested by Fabroni, a contemporary of theirs. He noticed that when two dissimilar metals, suspended in water, were made to touch, one of them became oxidized. The correctness of this theory was subsequently established by Sir Humphrey Davy, who showed that if the liquid in a voltaic pile is pure water, no current is obtainable from it, and that the activity of the pile "is in a great measure proportional to the power of the conducting fluid substance between the double plates to oxidate the zinc." The striking effects furnished by the use of the pile, such as the heating of conductors, the decomposition of chemical substances, and its physiological action,

excited great enthusiasm at the time. It brought fame and honour to its inventor. Napoleon created Volta a count and senator of Italy, granted him a liberal pension, and in many other ways bestowed signal favours on him.

In 1819 Volta retired to Como from his position in the University of Turin. From this onwards his relations with the scientific world ceased. In his retirement we are told he almost avoided the many travellers, who, attracted by his great fame, came to pay him homage. He spent a great part of his remaining years in the practice of his religion. He died in 1827 at the age of eighty-two. All Italy mourned his loss. Como celebrated his obsequies with great pomp. The professors and pupils of the high school, and all the leading inhabitants of the city and surrounding country joined in the funeral procession. A beautiful monument was raised to his memory in the picturesque village of Cammergo, from which his family originally came.

I can only briefly refer to Volta's earlier contributions to physical science. He invented the electrophorus and condensing electroscope. He devised a form of absolute electrometer. He studied the electrification of bodies in great detail, and under a great variety of conditions and form. He showed that the charge on a conductor is dependent on its shape ; for instance, in the case of two cylinders of equal surface, the longer one receives the larger charge. He obtained correct values for the dilatation of air with increasing temperature, and was the first to point out the necessity of enclosing the air in a perfectly dry flask. He discovered the action of flames in discharging electricity, and applied it to the study of atmospheric electricity. Chemists, too, are indebted to Volta for the invention of the eudiometer, and the discovery of the origin of marsh gas.

Returning now to the history of Galvani, it is sad to think that, unlike those of Volta, his closing years were

clouded by misfortune. Added to domestic bereavement and physical infirmity was a waning public interest in his great work, partly owing to the striking discoveries of his opponent, Volta. A man of great piety and conservative principles, he abhorred the changes wrought by the French Revolution, and when the Cisalpine Republic was established by French influence, he refused to take the oath of allegiance, and in consequence was deprived of his professorial chair. The next two years were spent in penury. At length, in 1798, the Republican Government decided to reinstate him, though he still refused to take the oath of allegiance. But it was too late. The great man died broken-hearted in December of the same year. Honours were showered on him after his death. In 1804 a medal was struck in his honour, and in 1814 a monument was erected to him in the University of Bologna. With regard to Galvani's religious views, it is perhaps hardly necessary to do more than to point out that, because his conscience forbade him, he refused to take the oath of allegiance to the so-called Cisalpine Republic in July. On this account he not only, as we have said, lost his professorship, or the stipend on which he had largely to rely for his living, but was brought to actual want during the two years he thus remained excluded from his chair. Of these circumstances his biographer, Professor Venturoli, says: "The great founder in electricity was deeply religious, and his piety clothed a heart that was not less affectionate and sensitive than it was intrepid and courageous. When called upon to take the civic oath in a formula involved in ambiguous words, he did not believe that he ought, on so serious an occasion, to permit himself anything but the clear and precise expression of his sentiments, full as they were of honesty and rectitude. Refusing to take advantage of the suggestion that he should modify the oath by some declaration apart from the prescribed formula, though it might still be generally understood that he had

taken the oath, he refused constantly to commit himself to any such subterfuge. It is not our duty here to ask whether his conclusion was correct or not. He followed the voice of his conscience, which ever must be the standard of duty, and it certainly would have been a fault to have deviated from it. It is sad to think that this great man, deprived of his position, saw himself, for an instant at least, exposed to the danger of ending his career, deprived of the recompense which he so richly deserved, and to which his past services to the State and the University had given him so just a title. This is all the more sad when we realize that the vicissitudes of his delicate health, much more than his age, now rendered such recompense doubly necessary. It is a gracious thing to recall, however, the noble firmness with which he maintained himself against so serious a blow. His courage is all the more admirable as one can see how absolutely without affectation it was. He was not ostentatious in his goodness, and did not permit himself to be cast down by the unfortunate conditions, but constantly preserved in the midst of adverse fortune that modest, imperturbable and dignified conduct which had always characterized him in the midst of his prosperity and his glory."

Alibert, another biographer, writing of him in 1801, shortly after his death, says: "We have seen already what was Galvani's zeal and his love for the religion which he professed. We may add that, in his public demonstrations, he never finished his lectures without exhorting his pupils to a renewal of their faith, by leading them always back to the idea of the eternal Providence which develops, preserves, and causes life to flow among so many different kinds of things."

But the most significant fact perhaps is one given in Professor Walsh's account of his life: "Before he died, he asked, as had his favourite poet Dante, whose *Divina Commedia* had been one of the pleasures of his life, and above all one of the consolations of his times of adversity,

to be buried in the humble habit of a member of the Third Order of St Francis. He is said to have valued his fellowship with the sons of the 'Little Poor Man of Assisi' more than the many honorary fellowships of various kinds which had been conferred upon him by scientific societies all over Europe."

RÉNÉ THÉODORE HYACINTHE LAENNEC

RENÉ THÉODORE LAENNEC

(1781–1826)

BY

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I. Science and Faith

Is there not a necessary antagonism between the uncompromising dogmas of the Church and the enquiring mind of the student of nature? Can a Catholic be a man of science? Is not scepticism the normal attitude of those engaged in research?

Such questions are frequently asked. It is one of the objects of the Catholic Truth Society in publishing this series of lives of Catholic men of science to give an answer.

It cannot be denied that many men in the first rank of science have been Catholics, and have remained Catholics to the end. But something more than this is required to answer fully the questions asked. It may be and is argued that an antagonism may really exist between Faith and Science, although the two exist together in one mind. It is unfortunately easy to divide the mind into two compartments, one of which is labelled Religion, and the other Science. The thoughts which occupy these compartments are not allowed to wander: there is no door between them. In short, contradictory statements can be and are believed by the same man. If a man hopes to be consistent he must not only have thoughts, but he must think about his thoughts. It is the latter duty that many men avoid. Thought No. 1 must not

be introduced to thought No. 2, for fear that they might quarrel; they must be kept separate and caged. Yet the mind is not a menagerie.

Perhaps, then, it is urged, the Catholic man of science puts Catholicism on one side of his mind and Science on the other, keeping both happy by keeping them apart; possibly he is broad-minded, in the sense that his mind is stretched laterally to afford space for incompatible ideas. Such criticism if unanswerable would, in the writer's opinion, rob this series of booklets of any argumentative value. If an individual agrees to differ from himself, one must be excused if one differs from him.

The facts of science are stubborn things, one cannot get round them, one cannot get past them, and if one attempts to get through them, one is damaged. The dogmas of the Church are equally stubborn; they are more immovable than the hills, for Faith can move the hills whilst it holds the dogmas firm. If it is here that an antagonism exists, the case is hopeless; we must either close our churches or our laboratories, or, better still, both.

But science does not consist solely of facts, it does not even consist mainly of facts. There is a large amount of theory mixed with a small amount of fact. It is with the theories of science and not with the facts that the dogmas of the Church may come in conflict. If, as the Catholic believes, dogmas are facts, it is only to be expected that they, like facts, should frequently be in conflict with theories. And this is a matter of no importance, for theories are not permanent, they are constantly changing, constantly disappearing. Nature says, I will give you ten thousand guesses at the truth: Man makes one; thus is a theory born. Just as we are all warned not to put our trust in princes, so the student of science is warned not to put his trust in theories. Yet theories have a function to perform, and that is to suggest fresh

experiments, these latter frequently dealing the death-blow to the theory that gave them birth. When theories claim absolute truth they cease to be fertile; they become the enemies not the friends of science. The theories of science are excellent parents of facts, but they are often short-lived.

What, then, is the attitude of the Catholic man of science? He has found and he will always find that there is no antagonism between the facts of science and the dogmas of the Church. He has found and he will always find that there are certain theories which are antagonistic to the dogmas of the Church. He has found and he will always find that where there is real antagonism it is the theory not the dogma that dies. He has not found and he will never find that his knowledge and his faith are incompatible.

Is his freedom of research restricted? Yes, if he be in search of theories. No, if he be in search of facts. Facts have no terrors for him; he is not afraid of fresh discoveries, for he has faith in the Creator of all phenomena. He does not ask himself, as many a sceptic does, what is the purpose of gaining fresh knowledge, for where does it lead? for he believes that each step gained in knowledge is a step towards perfection. Because he hears the voice of God in the Church, he is eager to catch every whisper of that voice in the world of nature. He knows that God is walking in the garden. He is not afraid.

A complex mechanism is presented to us for examination. Someone tell us that it is a mere fortuitous mixture of wheels and springs, purposeless and futile. It comes from nowhere, it is going nowhere, probably it is not going at all. Waste no time on it, there are better things to do. Let us eat and drink, for to-morrow we die. Another tell us that the mechanism is the work of a supreme artist, more than worthy of the study of a lifetime. Let us work and think, for to-morrow we live.

Faith, faith in something, is the essential stimulus to research. Work without faith is as impossible as faith without work. A man who believes nothing will never want to know anything. Thus the most foolish creed is better than no creed at all. It is wiser to believe that God is made of stone than to believe that stone made itself, since the first entails the belief that there is something in stone well worth investigating.

Is there an answer to the riddle of nature? The agnostic hesitates to reply. The Catholic says Yes, and by that very affirmative he is encouraged to proceed to discover that answer. His Catholicism is not an impediment but a spur. It does not say to him, Thus far shalt thou go but no farther; on the contrary, it tells him to push forward with all the power he possesses. The riddle can be solved, and he does not fear the solution. *Finis coronat opus.*

II. René Théodore Laennec

The present pages are concerned with a Catholic man of science to whom medicine owes an overwhelming debt for his discoveries of methods for the diagnosis of disease. René Théodore Laennec was born on 17th February 1781, at Quimper, in Brittany. His father was a lawyer whose acumen turned him more towards literature than law. His mother died when Laennec was six years old; and his early education was conducted by the Abbé Laennec, his grand-uncle, at Elliant, under whose tutelage four or five years were passed. He was then sent to continue his education with his uncle, Dr Laennec, at Nantes. Here he gained many prizes, and in addition to his normal studies he interested himself in the medical work of his uncle. It was observation on his own account rather than reading about the observations of others that attracted him. Here lies the distinction between the first-rate and the second-rate

mind. The clinical study of cases in the Military Hospital excited his keen attention. Although signs of brilliancy showed themselves in him, it is a fact that the early life of genius and the early life of mediocrity have frequently much in common—buds are much more alike than flowers. Even in later life some find a difficulty in distinguishing a fool from a genius; in early life it is well-nigh impossible.

In the year 1800, when he was nineteen years old, he proceeded to Paris, where he was to make his name resound throughout the world of medicine. Before scarcely a year had passed he was awarded the two first prizes in medicine and surgery by the University of Paris. In 1804 he wrote two theses on Hippocrates, the father of medicine in Greece.

The Paris school of medicine at that time exhibited its vitality by possessing two eminent men of strongly opposed views, Pinel and Corvisart. Pinel was a teacher of philosophic medicine, attempting to find the origin of disease by an analysis of the conditions of disease, best known to-day as the physician who freed the insane from the manacles that hitherto had been regarded as necessary restraints. The insane owe him much. Corvisart, who insisted on the absolute need of bedside investigation, was an exponent of the tradition of Hippocrates. Corvisart the physician because he was a clinician; Pinel the clinician because he was a physician.

Laennec was naturally drawn to Corvisart and became one of his favourite pupils, and Corvisart was an excellent master, for he possessed the rare power of stimulating others to investigate; he was a great teacher in that he induced others to teach themselves. Under him Laennec could freely indulge his enthusiasm for the study of the signs of disease in the living and the dead. Thus ten years passed.

In 1812 he became physician to the Beaujon Hospital in Paris. In 1816 he was appointed to the famous

Necker Hospital, and it was there his genius proclaimed itself. From all parts of the world students came to hear his lectures, for these lectures were stored with new thought, new wisdom, and new discoveries. A contemporary writes of him thus: "Laennec was almost an ideal teacher. He talked very easily, and his lesson was always arranged with logical method, clearness, and simplicity. He disdained utterly all the artifices of oratory. He knew, however, how to give his lectures a charm of their own. It was as if he were holding a conversation with those who heard him, and they were interested every moment of the time he talked, so full were his lectures of practical instruction."

Many a teacher fails apparently to interest even himself; Laennec's achievement of interesting others at every moment is therefore remarkable.

III. The Discovery of the Stethoscope

It is, however, in the method of auscultation that Laennec found his claim to fame. It should be explained that auscultation consists in listening to the sounds produced by the lungs and heart in health and disease. For the lungs and the heart, which are machines in constant action, have their language like all other machines. They talk in one way when all is well, and in another way, indeed in various other ways, when something is going wrong with the mechanism. Is it not in one of Charles Reade's novels that a character is introduced whose one use in life was to tell by the sound of the running grindstones whether they were safe or whether they were entering upon a state in which they would be a source of grave danger to all those working with them? At any rate, everyone who is in the habit of driving a motor-car or riding a bicycle is aware that he must use his ears at least as much as any other of his senses if he wants to proceed safely and comfortably on his journeyings. Such

persons rapidly learn to detect the differences between the normal sounds when the machine is running satisfactorily and the abnormal unaccustomed sounds which may arise from some trivial cause or may portend something seriously wrong with the mechanism. And so the driver or rider will stop his machine and get down to ascertain whether the disagreeable and unaccustomed sounds which have been annoying his ears are due to a nut which wants tightening if accident is to be avoided, or merely to a loose strap and buckle which may clack and clack and do no harm. In the same way the lungs have their own characteristic language in health and in disease. The gentle whisper of normal respiration may pass into the painful bubbling breathing of serious disease, a change audible and obvious even to the untrained observer. And between these extremes there are a thousand and one variations not similarly obvious to the non-medical man, but all conveying important information to the trained ear. Similarly with the heart, that untiring muscle which day and night pumps blood through the arteries, capillaries, and veins, it also has its normal language and its cry of distress. To some extent these sounds can be investigated by the method of placing the ear directly on the chest-wall and listening to what is going on inside. Such a plan, however, presents a variety of disadvantages, some of which will be alluded to further on, and is at anyrate open to this grave objection, that the sounds can neither be as clearly distinguished nor as sharply located by the method of immediate auscultation as they can be by that of mediate.

In other words, to use plain language, the doctor can make out what is wrong better by listening through some form of stethoscope than he can by placing his ear directly on the patient's chest. Laennec's great reputation depends on the fact that he discovered a method of hearing these sounds clearly and that he interpreted

them correctly. Laennec, in short, was the inventor of the stethoscope, that instrument which most of us have felt upon our chests whilst wondering vaguely what the doctor heard, if, indeed, he heard anything.

Laennec's stethoscope was tubular and of wood—in fact, a reproduction in wood of the quire of paper the story of which is shortly to be told; and on this model for many years stethoscopes were constructed. Many people will remember having seen such instruments stuck in the interior of the professional silk-hat of the medical adviser, that being a favourite place to carry the implement. Nowadays a binaural instrument, consisting of rubber or metal tubes or both, with earpieces for both ears of the doctor and a chest-piece to rest on the patient, is more usual, but, whichever form we come in contact with, the principle is the same. It is the principle of the conduction of sounds from the chest of the patient to the ears of the physician by means of an intermediate channel. That is the stethoscope, and it remains and must ever remain one of the most valuable implements of physical diagnosis in the armamentarium of the medical man. We may place it beside the clinical thermometer, also the discovery of a French physician, as one of the two most commonly used instruments for diagnostic purposes.

As to the discovery of the stethoscope, Laennec must be allowed to speak for himself. "In 1816," he writes, "I was consulted by a young person who was labouring under the general symptoms of diseased heart. In her case percussion and the application of the hand (what modern doctors call palpation) were of little service because of a considerable degree of stoutness; the other method, that namely of listening to the sounds within the chest by the direct application of the ear to the chest wall, being rendered inadmissible by the age and sex of the patient." Some will remember the irritation caused by the direct method in the case of a whiskered physician.

Laennec continues: "I happened to recollect a simple and well-known fact in acoustics, and fancied it might be turned to some use on the present occasion. The fact I allude to is the great distinctness with which we hear the scratch of a pin at one end of a piece of wood on applying our ear to the other.

"Immediately on the occurrence of this idea I rolled a quire of paper into a kind of cylinder, and applied one end of it to the region of the patient's heart and the other to my ear. I was not a little surprised and pleased to find that I could thereby perceive the action of the heart in a manner much more clear and distinct than I had ever been able to do by the immediate application of the ear.

"From this moment I imagined that the circumstance might furnish means of enabling us to ascertain the character, not only of the action of the heart, but of every species of sound produced by the motion of all the thoracic viscera, and consequently for the exploration of the respiration, the voice, the râles, and perhaps even the fluctuation of fluid effused in pleura or pericardium. With this conviction I forthwith began, at the Necker Hospital, a series of observations from which I have been able to deduce a set of new signs of the diseases of the chest. These are for the most part certain, simple, and prominent, and calculated, perhaps, to render the diagnosis of the diseases of the lungs, heart, and pleura as decided and circumstantial as the indications furnished to the surgeons by the finger or sound in the complaints wherein these are of use."

The claim which Laennec here makes for the value of his discovery is under-estimated. There are few men who, when they have discovered one thing, do not think they have discovered all things. Laennec was one of these few; he possessed a mind which could value correctly the consequences of his own work. He occupied two years in studying not only the possibilities but also the limitations of the stethoscope; after this investiga-

tion he sent an account of his work to the French Academy of Sciences. Three members of the Academy were selected to investigate his discovery, Doctors Pelletan, Portal, and Percy. These must have been somewhat remarkable men, for they adopted the unusual course of reporting favourably on the new discovery. Even so, they showed the conservatism which is by no means absent in medicine by an extreme caution in their approval. The faintness of their praise leaves them but a faint reflection of Laennec's fame.

Before Laennec's time, the diseases of the lungs and heart in man were in much the same nebulous condition as diseases of cattle were till recently. A disease of the lungs, accompanied by fever, was "lung fever," and there was an end of it. A disease of the heart was clearly "heart disease," and what more could be said? But Laennec changed all this. He showed that disease of the lungs took many forms, and that these forms could be distinguished from each other. Again, it is to Laennec's introduction of auscultation by the stethoscope that medicine owes its knowledge of heart diseases and their diagnosis. The Irish school of medicine applied his method with brilliant results in the cardiac lesions which he himself had failed to interpret.

Laennec's method of auscultation rapidly drew students from even the most distant parts. The simplicity of the discovery was in itself an attraction; added to this there was self-evident utility in calling in the sense of hearing as an aid to diagnosis.

"A sense was lacking in medicine," wrote Builland, "and I would say, if I dared, that Laennec, the creator, by a sort of divine delegation, of a new sense, supplied the long-felt want. The sense which medicine lacked was hearing. Sight and touch had already been developed in the service of medical diagnosis. Hearing was more important than the other two senses, and in giving it to scientific medicine Laennec disclosed a new world of

knowledge, destined to complete the rising science of diagnosis." "Laennec in placing his ear on the chest," said Henri Roger, "heard for the first time in the history of disease the cry of suffering organs. . . . His ear opened to the mind a new world in medical science."

It is interesting to record that Laennec made his own stethoscopes, sometimes constructed them ornately. One probably made by him is treasured in the museum of the College of Physicians of Philadelphia.

Three years of investigation of auscultation of the lungs and heart preceded Laennec's great book on the subject, a book which has become a classic. Dr Austin Flint, the elder, one of the greatest diagnosticians of America, writes of it thus: "Suffice it to say here that, although during the forty years that have elapsed since the publication of Laennec's works the application of physical exploration has been considerably extended and rendered more complete in many of its details, the fundamental truths presented by the discoverer of auscultation not only remain as a basis of the new science, but for a large portion of the existing superstructure. Let the student become familiar with all that is now known on the subject, and he will then read the writings of Laennec with amazement that there remained so little to be altered or added."

One of the sure signs of genius is its finality, and the work of Laennec exhibited this sign to a marked degree.

It was not only in the diagnosis of thoracic diseases that Laennec left his mark. His work was over a most extensive field. Every subject he touched he illuminated. For instance, he investigated the influence of alcoholic excess on the liver, and it is to him we owe much of our knowledge of "cirrhosis of the liver," a term he himself originated.

In this connection non-professional readers may be reminded of the dire effects upon the liver of constant

alcoholic excess, and particularly of continuous and excessive drinking of spirits such as gin, whisky, brandy, and the like. Under normal circumstances the liver presents a smooth, shining surface, even and unbroken by elevations. When affected by cirrhosis this appearance is entirely altered, and the organ, instead of being smooth, becomes studded with little knobs or elevations something like the studded or "non-slip" tyres with which we are all familiar on motor-cars. This changed condition is sometimes known as "hob-nail liver" or "gin-drinker's liver," and more scientifically as "cirrhosed liver." What has happened is this: The liver normally consists of a very large amount of glandular tissue, with, of course, the necessary blood-vessels, ducts, and nerves. With this is associated a small amount of fibrous tissue, but this last element exists, relatively, in very small proportions when compared with the great bulk of the organ, which consists of glandular cells. Under the influence of alcohol, and especially, as above mentioned, of alcohol in the form of what are commonly called "spirits," the fibrous tissue takes on an active growth. It not only does this, but, as it increases in quantity in proportion to the glandular tissue, it contracts and causes islands of glandular tissue to project between the meshes of the network which it forms. These islands are the elevations or "hob-nails," and the depressed portions between are formed of fibrous tissue. It is perhaps hardly necessary to point out that such grave changes in the structure and size of the liver—for as a whole it becomes shrunken and contracted—cannot take place without equally grave effects on the general health of the victim of this disease. With these effects we are not concerned here, for this is neither a treatise on hepatic pathology nor a temperance tract. Suffice it to say that the points just alluded to were in large measure elucidated by Laennec, and that for this addition to medical knowledge alone his name would

have deserved an honourable place in the history of medicine. As it is, the greatness of the discovery of the stethoscope has overshadowed his other claims on our gratitude; but in any account of his life, however brief, some mention of his services to medicine in the direction now indicated must not be omitted.

After some twenty years of devotion to medical science Laennec's health gave way, and he retired to the country for two years to recuperate. Although being fully assured that a return to work would entail a second illness, he nevertheless returned to Paris at the end of this time, and again took up his hospital duties. A year later he was appointed Professor of Medicine in the College of France, and subsequently to the chair of clinical medicine at his old hospital, La Charité.

IV. His Personal Character

Something should be said as to the personal character of the man, for we are concerned not only with Laennec as a leader of medical thought, but with Laennec as a Christian, and there can be no doubt that throughout his life he was an example to others of what a Christian gentleman should be. His intimates have declared that they never found him in an angry mood or even in an impatient one; he was always calm. In his discussions with others this calm never failed him, whatever provocation his opponents gave him.

He was a true friend, always ready to give assistance, and, what is more remarkable, he injured no one. An intimate friend of his has placed on record that he had never heard Laennec express by a single word, or even by the slightest insinuation, anything that might seem to indicate pride in what he had accomplished, or that might provoke a listener to say anything in his praise.

One of his biographers, Dr Henri Saintignon, writes of him thus: "I have shown in the course of this life

just what was the character of Laennec and his intellectual and moral qualities, so that it will not be necessary for me to dwell at length on this subject in concluding. His great piety, which had never been abandoned from his earliest infancy, was his main guide during all his life. Without ostentation, yet without any weakness, absolutely ignoring human respect, he obeyed with utter simplicity the prescriptions of his faith. While he did not conceal his convictions when during the First Empire they might have proved a source of lessened esteem, or positive prejudice, he made no noise about them when under the Restoration they might have proved the means of advancement and of fortune. He had not in the slightest degree what is so often objected to in devoted persons, namely, the love of making proselytes. The words of Professor Desgenettes might very well have been applied to him: as he did not believe himself to have any mission to lead others to his opinions, he limited himself to preaching by example. The reproach of being rabidly clerical or propagandist, which was urged against him when he first became a member of the faculty of medicine, was absolutely unjustified. Laennec never occupied himself with politics nor with religion in public. As a physician he devoted himself exclusively to his profession, receiving at his clinic all those who desired to follow his teaching, whatever might be their opinions or their beliefs."

We gain some knowledge of the man when we are told that he often refused to see rich patients from lack of time, but he was never known to refuse to see a poor patient. A genius is rare, but a genius who shines not only with the light of knowledge but also with the light of charity is far more rare. Laennec, indeed, was a true son of the Church, and a glory to the land that gave him birth.

"His religious principles, imbibed with his earliest knowledge, were strengthened by the conviction of his

maturer reason." Thus wrote his contemporary, Bayle, concerning him. "His life," said another, "affords a striking instance among others disproving the vulgar error that the pursuit of science is unfavourable to religious faith." As an instance of his piety, we are told that once on his way to Paris, in company with his wife, they were thrown from their carriage. On resuming their seats, Laennec said, "We were at the third decade"; and they resumed their rosary at the place where they had been interrupted by the accident.

The sceptic may well find Laennec an insoluble problem; to him a mature faith only implies an immature mind, yet Laennec's mind was of the first order. To the sceptic the teaching of the Church is but a skilful blend of poetry and superstition, yet Laennec accepted this teaching, and he possessed an intellect far keener than the vast majority of unbelievers. The sceptic finds in Laennec an addition to the vast number of riddles which he cannot solve. He cannot doubt Laennec's genius, and he cannot doubt Laennec's faith, and, as he thinks, the two are mutually destructive. The sceptic finds more mysteries than the Christian, but the mysteries of the unbeliever depend on his eyes being closed, whilst those of the Christian depend on his eyes being open.

The world is not surprised to find a sane mind in a sane body, and the Catholic is not surprised to find a sane mind in a sane soul; he is, indeed, surprised to find the contrary. It may be difficult to understand the origin of Laennec's genius, but it is not difficult to understand how genius and faith existed together within him.

V. His Last Days

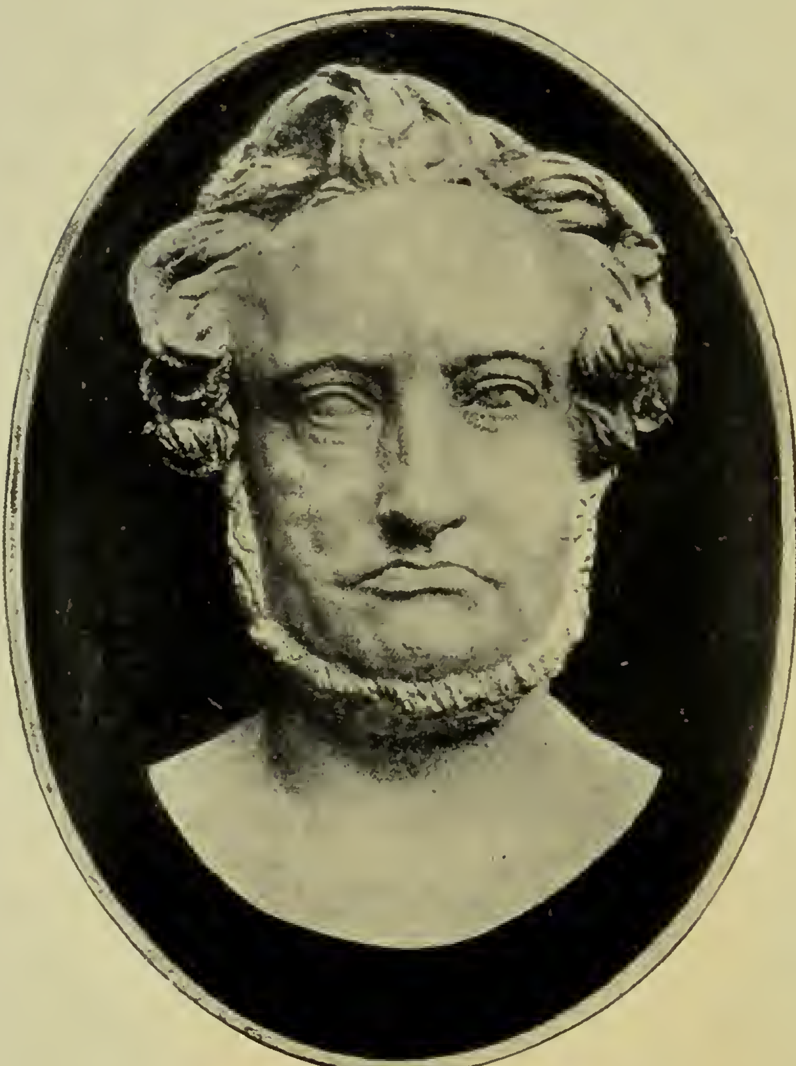
Before long, after his return to Paris, Laennec's health again began to fail. Overwork and a constant association with tubercular patients were the probable cause of this misfortune, for he himself became a victim of tubercle

of the lung. His condition rapidly grew worse, and a release from work became imperative. He returned to his native Brittany, where he developed all the signs of phthisis. Various remedies were tried, but without success, the treatment of tuberculosis in those days being in a very primitive condition. It could be said of him, as it was said of his Master: "He saved others, but himself he could not save." His charity to the poor, always a feature of his character, became very marked; and his great anxiety was to avoid giving trouble to others.

The curé of Kerlouanec paid him frequent visits and administered to him the consolations of religion. All knew that the end was very near. Shortly before his death he removed the rings from his hand, saying to his wife, "It will not be long before someone else would have to do this service for me, and I do not wish that they should have the trouble." Two hours later, on August 13, 1826, Laennec breathed his last. Thus passed from earth a pioneer of medicine and a true Catholic.

A man who spent his life and strength in alleviating the suffering of others; a man whose lucid mind was eager and able to solve the mysteries of nature; a man whose clear soul reflected the mysteries of God: such a man rests in Laennec's grave.

[The writer wishes to express his indebtedness to the essay on Laennec by Dr J. Walsh in *Makers of Modern Medicine*, published by the Fordham University Press, New York.]



JOHANNES MÜLLER

JOHANNES MÜLLER

(1801–1858)

BY G. A. BOULENGER, D.SC., PH.D., F.R.S., &C.

IN the illustrious man whose life and work we shall endeavour briefly to sketch, we have an example, rare in the history of Science, of one who rose to fame at the very outset of his career, and, after holding for a quarter of a century the highest position to which a man of science can attain, disappeared suddenly from the scene of his triumphs at the very moment when doctrines in so many respects opposed to his were to revolutionize biology, and, for a time, to carry everything before them.

Unlike Gregor Mendel, who died nearly thirty years ago, venerated in his monastery but almost unknown to the scientific world, and whose name and work now fill more columns in the *Encyclopædia Britannica* than are devoted to any of his contemporaries in science, he reaped in his lifetime all the recognition and honours which a well-earned reputation can ensure.

He has been called the Cuvier of Germany, but it is difficult to say in which of the two great branches of knowledge he cultivated, Zoology and Physiology, he has shone the brighter.

Johannes Müller was born on July 14, 1801, at Coblentz, then under French rule—the inverse of the great Cuvier, who was born and received his first education on French territory, then occupied by the

Germans. His father was a rather well-to-do shoemaker, whose ambition for his eldest son went no further than to destine him for the leather trade. But after some years' schooling in the so-called Jesuit College at Coblentz (1810–1815)—replaced, after the expul-(1816–1818)—the boy showed such exceptional aptitude for study that, through his enlightened mother's influence, the father's intention was abandoned, and young Müller, after serving a year in the army as a volunteer, was sent to the newly founded University at Bonn. Whilst there, at the age of eighteen, he evinced a strong bent towards natural science, which gradually took hold of him to such an extent as to make him abandon his intention of devoting himself to the service of the Church—a thought which had been present to his mind since early childhood, and which had been encouraged by his pious and affectionate mother. Even then, however, he wavered, and it is said he shut himself up in his room for several days to arrive at a decision, with the result that he chose Medicine, and two years later was the recipient of the first prize awarded by the Medical Faculty of the University, a singular honour for a lad of twenty.

His first publication, in Oken's *Isis* for 1822, on locomotion in insects, spiders, and centipedes—which, in enlarged form, constituted his thesis for the degree of M.D. (Dec. 14, 1822)—was so permeated with the ideas of the new school of Natural Philosophy, then in fashion through the influence of Goethe and Oken, and later, in this country, of Owen, that, on freeing himself after a time from its doctrines, he so regretted this work as to make all possible efforts to buy up any copies in the market for the purpose of burning them. It is somewhat remarkable that he should have chosen as his first subject of investigation a group of creatures for which he felt an innate repulsion. As a

boy he was known for his dread of spiders, a dread which his schoolfellows lost no opportunity of arousing in order to tease him. Yet a few years later, when a soldier on watch duty on the old walls of Coblentz, he whiled away the time by observing the movements of spiders, and even went so far as to keep some in boxes for weeks, with the object of better understanding the principles of their locomotion, slower in individuals weakened by a long starvation.

Müller was in the second year of his university studies when he lost his father, and as the shoe-making business became a failure, he found himself in such reduced circumstances as threatened to interfere with his career; but living with the strictest economy, he managed to get along, helped by friends, among whom was Rehfues, one of the most influential men of the University, who even obtained money for him from the Catholic Theological Faculty. He was thus enabled to undertake a journey to Berlin, in 1823, where, on the recommendation of Rehfues, he enlisted the support of the Minister of Education, von Altenstein, a support which was continued to him ever after and was to lead to his appointment as a professor in the Berlin University. Great was the young man's joy at finding himself in the Prussian capital among the treasures of the anatomical collections over which Rudolphi presided, and of the Zoological Museum under Lichtenstein, as well as at the genial reception which Rudolphi extended to him. There he laid the foundation for his work on the comparative physiology of vision, and prepared his first contribution to morphology, dealing with some points in the anatomy of an orthopterous insect, *Phasma*.

In 1824 he was appointed Privatdocent in Bonn University, to lecture on anatomy and physiology. The work on vision in man and animals, which, as we have said, was begun in Berlin, came out in 1826, as a

separate book of 426 pages, published in Leipzig, full of well-observed and important facts and judicious reasoning, marshalled in masterly fashion. This great work was soon followed by a smaller essay on the phantasmal phenomena of vision, on which subject he had much experimented upon himself, to the detriment of his health.

The success of his teaching was such that, at the age of twenty-five, he was raised to the rank of Professor in Extraordinary, without, however, receiving the emoluments attaching to the post. He tried to increase his income by practising as a doctor, but without much success. Poor as he was, he nevertheless married in 1827; and trying to meet this increase in responsibility by excessive work, he nearly came to grief. A serious nervous illness seized him; he fancied himself nearing paralysis and death; and gave up all his occupations. Fortunately, his friends came to the rescue ; he obtained from the Minister of Education a long leave of absence and a grant of money with which he hired a horse and trap, and, in company with his devoted wife, started on a tour on the Rhine and in South Germany, from which he returned some months later fully restored to health.

Having thus regained his extraordinary working powers, Müller investigated the structure of the eye in Invertebrates, the nervous system of animals in general, and of scorpions, spiders, and centipedes in particular the embryos of man and mammals, the genital organs of animals, discovered the pronephros (primitive kidney) in Batrachians, the minute primordial filament, since known as the Müllerian duct, which gives rise to the oviduct or Fallopian tube, and, in 1830, brought out his celebrated work *De Glandularum*, &c. This work at once placed him among the foremost anatomists, and was rewarded by the Paris Academy of Sciences with a gold medal. The intimate structure of the

secreting glands is in this work investigated throughout the animal kingdom, from the embryonic to the perfect state, and their relation traced to the blood-vessels and ducts, resulting in the finest piece of work of the kind since the days of the great Italian physicist and anatomist, Malpighi (1628–1694).

No sooner had all this been accomplished than Müller threw himself into a new department of experimental physiological investigations on the nerves, the blood, and the lymph (1831–1832). In connection with his work on the lymph, he made a very important discovery, which formed the subject of his first communication to our Royal Society (1832), of which he was to become, a little later, one of the most distinguished foreign members, and, subsequently, a Copley medallist. This was the discovery of the existence of lymph-hearts in Batrachians.

Considering how much the frog's anatomy had been studied, and how attention had already been drawn to the lymph-sacs, situated between the skin and the muscles, it is remarkable that these hearts, which are so conspicuous now that we know where to look for them, had altogether escaped observation. In the subcutaneous sacs of a frog, the lymph is usually found in abundance, and will flow pretty freely when the skin is cut, continuing fluid for ten minutes and then coagulating. By this means, as Müller observed, lymph can be exhibited to students, a matter of some importance, for medical men had then very rarely an opportunity of seeing it in the whole course of their lives. Now these lymph-sacs are provided with two pairs of pulsating organs—lymphatic hearts as Müller called them—only the posterior pair of which was first discovered by the great physiologist, the anterior pair being found a little later simultaneously by himself and by the Italian Panizza, thus showing by what a series of steps comparatively simple facts come to be ascer-

tained, even under the scalpel of the ablest investigators. Müller observed that the contractions of these hearts are neither synchronous with the motions of the heart proper nor with those of the lungs, but are peculiar to the organs themselves; for they continue after the removal of the heart and even after the dismemberment of the animal. He was on this occasion able to set forth clearly the movements of the lymph and its connection with the venous system.

Müller's first work on the natural history and comparative anatomy of Vertebrates dealt with the Batrachians, called by him *Amphibia nuda*. His discovery, in 1831, of the spiracula in a young Cæcilian preserved in the Leyden Museum, settled the then controverted question as to the position of these wormlike creatures in the system, as the fact that they undergo metamorphoses had not been established before. He also contributed about the same time to our knowledge of the structure of this class of animals, and based a classification of the frogs and toads on the condition of the auditory organ, a classification which, however, did not reflect their natural relationships. This work was chiefly carried out in the Paris Museum, the duplicates in which establishment were placed at his disposal by Cuvier. In a letter to his friend Retzius, dated November 14, 1831, Müller writes how delighted he was with the treasures in that Museum, then the greatest in the world, and how pleasant Cuvier had made himself to him, telling his assistant Laurillard "Donnez à ce Monsieur tout ce qu'il voudra." The intention which Müller then expressed of publishing monographs of some of the more interesting genera of frogs was never carried out.

Müller's reputation as an investigator and teacher had risen high, when the death of Rudolphi in November, 1832, left the chair of Anatomy and Physiology in Berlin vacant, and opened a new field

for his activity. At Easter, 1833, when only thirty-one years of age, he replaced Rudolphi as Professor in Ordinary, and a year later he was elected into the Berlin Academy—a well-earned reward for such a remarkable career, and a good fortune for the University, of which he was to be the shining light for exactly a quarter of a century.

He had not been many months in Berlin when appeared the first part of his celebrated *Handbuch der Physiologie*, completed in 1840, which was to be for many years the standard treatise on this vast subject, went through several editions, and was translated into English and French. This was regarded as the most valuable general work on physiology which had appeared since Haller's *Elementa* (1757-1766). Indeed, as stated in the obituary notice which appeared in the Proceedings of the Royal Society for 1858, "the two great physiological writers have much in common. In both we see the same earnest purpose of placing the doctrine of physiology on a basis of fact, the same constant endeavour to extend and consolidate this foundation, or test its validity, by materials and methods placed at their command by their accomplishment in the cognate and collateral sciences. Anatomy, human and comparative, experiments on animals, chemistry, and physical science in its various departments, are all brought to bear in the investigation of physiological truth.

"Müller's work is, moreover, enriched throughout with the fruits of the author's own observation and experimental inquiry, which are, sometimes, it is true, given with a detail better suited for a separate memoir than for a chapter in a handbook, but which signally enhance its value as an original source of information. Almost every part of the book affords evidence of this, but it is enough to refer specially to the examination of the blood, the disquisition on the nervous system, and the valuable experimental investigations

on the voice and hearing. Here, as in his other writings, it is characteristic of Müller that he takes nothing on trust ; every statement, whether of matter of fact or doctrine, is thoroughly sifted. Difficulties, however perplexing, are never evaded or slurred over ; defects, however they may deface the picture to be presented, are never disguised. Every question is resolutely attacked ; the result, whether success or failure, is honestly told ; and there is no yielding to the temptation, so powerful with writers of systems, of rounding off a rugged subject with smooth plausibilities."

On the completion of his Handbuch, Müller received from the King of Prussia the gold medal for Art and Science. Müller is unquestionably the greatest physiologist of the first half of the last century, and on the occasion of the award of the Copley Medal in 1854, the President of the Royal Society observed that " no one has borne a more conspicuous part in the advancement of physiological science for the last quarter of a century than Johannes Müller." Yet no less great are his merits as a morphologist. So greatly have the domains of these two branches of biology been extended, to no small extent through his own influence, that he will no doubt have been the last in the history of Science to combine eminence in both animal physiology and morphology. He himself, in the second half of his career, felt the burden to be too heavy for his shoulders, and as he advanced in years he devoted himself more and more exclusively to the latter department, leaving the former to his eminent pupil, E. du Bois-Reymond, on whom fell the honour of delivering before the Berlin Academy of Sciences the beautiful obituary discourse from which the writer of these pages has freely drawn in preparing this sketch.

It has often been objected to Müller that, in the

course of his physiological demonstrations, he abstained from vivisection of warm-blooded animals at a time when these were constantly sacrificed in other schools, and where hands stained with the blood of dogs and rabbits were for a time regarded as one of the necessary attributes of an up-to-date physiologist. Although he did not hesitate to perform occasional vivisections in his private laboratory, he felt diffidence at availing himself of such a privilege except for the solution of problems of great importance.

Absorbed as he was in so many occupations, Müller yet found time to contribute, from 1834 to 1844, annual reports on the progress of anatomy and physiology, which, although he was helped in later years by collaborators, must have entailed an enormous amount of reading and hunting up, and the fact that he added critical remarks to the contributions which he reviewed must have been the cause of no small amount of unpleasantness. These reports were published in the *Archiv für Anatomie, Physiologie und Wissenschaftliche Medicin*, of which he took up the editorship in 1834. Long known as *Müller's Archiv*, this important periodical, since divided into two sections, is still in existence.

Availing himself of the great discovery of his pupil Schwann in 1837, Müller applied the microscope to the investigation of morbid growths in man, and started the work which, continued by Virchow and other pupils, was the foundation of the celebrated Berlin school of pathological anatomy. Schwann, it may be mentioned in passing, the founder of the cell-theory, was all his life a faithful Catholic, and held for some time a chair in the University of Louvain, where cellular biology was to occupy so important a place through the impulse of Carnoy—a position which has been maintained to the present day under the teaching of Canon Grégoire, one of the most distinguished of modern cytologists.

From 1836 starts a new era in the scientific work of Müller, that of publications on the morphology and classification of Vertebrates, especially Fishes, which for a few years were the chief object of his researches. It was no small surprise to his contemporaries to find the great physiologist suddenly taking the first place among systematic ichthyologists. Whilst finishing his text-book of physiology, his attention was attracted to a singular type of fish from New Zealand and the Cape of Good Hope, somewhat allied to, but even lower in organisation than the lampreys, which he named *Bdellostoma Forsteri* (the *Petromyzon cirratus* of Forster), and which, as he said, was of particular importance to the morphologist as representing the lowest type of fish, thus standing at the very base of the Vertebrate branch. He soon found out, through his friends the Scandinavian naturalists Eschricht and Retzius, that a closely related form, the hag-fish or borer, *Myxine glutinosa*, was to be procured in plenty from the North Sea, and thus provided with ample material he started on a series of papers on the Myxinoids, which extended over a period of eight years (1836–1843), the whole organisation of these fishes being dealt with in succession. The Cyclostomes, or Marsipobranchs, embracing the hag-fish and the lampreys, had hitherto been regarded as a group of cartilaginous fishes related to the Selachians, or sharks and rays. Müller was able to show how fundamentally they differ from them. They are now considered by many authorities as deserving to be removed from the fishes to constitute a separate class (Agnatha).

Müller was soon to find a still lower type of Vertebrates, on which to throw the light of his acumen, the *Amphioxus*. The history of this little creature is an interesting one.

It was described and figured for the first time in 1778 by the German zoologist Pallas, from a specimen

preserved in spirit which had been obtained on the coast of Cornwall. Pallas took it for a slug, and named it *Limax lanceolatus*. Nothing more was heard about it until 1834, when an Italian naturalist, Costa, rediscovered it in the Gulf of Naples, and regarding it as a fish allied to the Cyclostomes, and mistaking the curious tentacle-like cirri which form a fringe round its mouth for respiratory filaments or gills, proposed for it the misleading name *Branchiostoma*. It was next found on the English coast by Yarrell, who described it in 1836 under the name of *Amphioxus*, which name, against the rule of priority, has been adopted by most subsequent writers. A little later the study of this remarkable animal, which has a wide distribution, was taken up by Goodsir, of Edinburgh, by Rathke, of Königsberg, and by Johannes Müller, whose work deserves to be regarded as a masterpiece. Curiously, the publications of these three authors appeared in the same year (1841). Müller had himself collected a quantity of specimens in Sweden, and later he found at Naples enormous numbers, which could be easily picked up when bathing, and, after making renewed observations on living specimens, preserved a couple of thousand in spirit. He agreed with Costa in pronouncing Amphioxus to be a fish, which although somehow allied to the Cyclostomes differs from them to a greater extent than a fish differs from an amphibian. A few years later (1847) he was able to add an account of the larval form from a specimen obtained by him at Helsingfors.

In 1866 the Russian zoologist Kowalevsky published the results of his researches on the development of Amphioxus and of the Ascidians (sea-squirts), and established the relationship existing between these two types, the latter of which had hitherto been placed near the molluscs or near the worms. Kowalevsky showed that notwithstanding the great differences that

separate them in the perfect condition, their embryonic development is very similar, the Ascidian larva being provided with an elastic rod known as the notochord, like Amphioxus and all Vertebrates, at least in the earliest stages, and henceforth Amphioxus was removed from the Fishes and the Ascidians from the Invertebrates to form a division, *Protochordata*, of the great phylum *Chordata*, which embraces besides all Vertebrates, from Fishes up to Mammals.

Although Müller did not hit off the exact systematic position of the puzzling Amphioxus, yet he laid the foundation of an accurate knowledge of its structure and development, which was to be the starting-point of subsequent investigations, and here again he left his indelible stamp on one of the greatest zoological discoveries of the nineteenth century. Space forbids entering here into an account of the disputes to which this Amphioxus has given rise among evolutionists, some regarding it as a connecting link between Invertebrates and Vertebrates, others endeavouring to demonstrate that it is derived by degeneration from the latter.

The work accomplished on the Cyclostomes had led Müller to a study of the type of fish next in order, and with the assistance of his pupil Henle, who had previously occupied himself with the Torpedoes or electric rays, he brought out a large illustrated book dealing with the Plagiostomes, or sharks and rays, from the systematic standpoint—a book which is still in the hands of all who have to study the external characters of this important group of fishes. He also contributed an interesting paper on a dog-fish, *Mustelus lævis*, remarkable for the connexion of its fœtus with the uterus, a fact observed by Aristotle, but not verified since.

The work by which the great naturalist is best known as an ichthyologist is his memoir on the structure and limits of the Ganoids, followed by a general classification of the Fishes (1846).

As the outcome of his epoch-making researches on fossil fishes, Louis Agassiz had proposed (1833) a new classification based on the structure of the scales (Placoids, Ganoids, Cycloids, Ctenoids), which, although it expressed roughly the principal steps in the evolution of fishes from the oldest to the most recent types, was unsatisfactory in its details, and could not be applied without violence to the most obvious relationships of many types brought together under three of these primary divisions. This was particularly felt in the case of the Ganoids, which included, besides the forms with rhombic, enamel-coated bony scales, which are so strikingly characteristic of the palæozoic and mesozoic periods, the Sturgeons, the Silurids or Cat-fish, the File-fish and Coffer-fish, the Sea-horses and Pipe-fish, &c.

Müller undertook to put order into this chaos by instituting a thorough examination of the anatomy of the living representatives of Agassiz's Ganoids, with the object of assigning to the group more definite and restricted limits. He showed that the Sturgeon and Polyodon agree in the structure of the heart and intestine, the disposition of the optic nerves, &c., with *Polypterus* and *Lepidosteus*, the only living forms clad with true ganoid scales, whilst the other forms associated with them by Agassiz had to be removed from the group and distributed among the soft-rayed and spiny-rayed Teleosteans. The North-American *Amia* was, however, overlooked by him, and left with the Clupeids (Herring family), where Cuvier had placed it; it was Carl Vogt who discovered, at the close of Müller's investigations, that *Amia* also agreed with the new definition of the Ganoids. Müller further failed to seize the relationship which is now admitted to exist between the Lung-fish or Dipnoans (*Lepidosiren* and *Protopterus*) and his Ganoids. *Ceratodus*, it is true, was then only known from fossil teeth, the discovery of the living *Neoceratodus* in Queensland dating from 1870. The

differences in the skeleton which separate *Polypterus* from the other Ganoids were not accorded sufficient weight, as was recognized later by Huxley and by Cope, who established the order Crossopterygians, now usually regarded as of equal rank with the Ganoids and the Dipnoans. Recent discoveries in both fossil and recent forms have also proved fatal to Müller's definition of the Ganoids, and the barrier which was supposed to separate them from the lower Teleosteans has in consequence to some extent broken down. At the present day few authorities agree exactly as to what forms constitute the order Ganoids.

In dealing with the other groups of fishes, Müller introduced many improvements in Cuvier's classification, which Agassiz's had not succeeded in supplanting. He was the first to establish the great and most natural family Characinidæ, the components of which had been previously distributed among the Salmonids and Clupeids, at the same time pointing out their affinity to the Cyprinids and Silurids, although failing to realize their still closer relationship to the Gymnotids, which he left near the Eels. Abandoning the division of Agassiz into Cycloids and Ctenoids, he still attached too much importance to this scale-character in some of his groupings—in the Pharyngognathi, for instance, an entirely artificial association of Teleosteans with united lower pharyngeal bones, the establishment of which he looked upon with some pride, but which was soon to be upset. His application of the character of the presence or absence of a pneumatic duct to the swim-bladder for a definition of higher groups was a step in advance, although the subsequent discovery of exceptions has lessened its utility from a taxonomic point of view.

Like all systems, Müller's classification of fishes has been replaced by others, and little remains of it in the modern attempts at a phylogenetic arrangement; but it

had endured a longer spell than any of its predecessors, to which it was superior in the expression of natural relationships. No higher compliment could be paid to any of our modern classifications.

With the collaboration of one of his assistants—Troschel—Müller started, in 1845, a fine illustrated work—*Horæ Ichthyologicæ*—containing descriptions and figures of various fishes, the third and last part of which appeared in 1849.

In the early forties Müller threw himself with enthusiasm into the study of bird anatomy, with the special object of improving the then very artificial classification which was based partly on the mode of life, partly on external characters, such as the bill and feet, which reflected the habits. As Audubon had said, the time had come when the results obtained from an inspection of the exterior alone had to be laid aside. In dealing with the enormous group of the Passerines, Müller availed himself for the first time of the condition of the larynx and song-muscles to establish three great tribes among them—an arrangement which, whatever its shortcomings, constituted a great step in advance. His work did not appear in complete form until 1847. In the words of the late Professor Newton, this very remarkable treatise forms the groundwork of almost all later or recent researches in the comparative anatomy and consequent arrangement of Passerine birds, and though it is certainly not free from imperfections, many of them, it must be said, arose from want of material.

In criticizing the taxonomic work of Johannes Müller, it must be borne in mind that the almost universal acceptance of the evolutionary or derivation theory, of which we find a hint in the writings of St. Augustine, has completely revolutionized the principles of classification. The motto inscribed on one of Linnæus's portraits, *Deus creavit, Linnæus disposuit*, expresses the conception of his contemporaries. The

aim of the classificator was to discover the order and sequence which had characterized the separate acts of the Creator of all things. Hence the purpose of the taxonomist was to avail himself of such characters as stood in direct relation to the economy of the creatures; and the so-called natural systems which soon followed and replaced Linnæus's tentative and artificial scheme, were mainly physiological, the characters selected as of the highest importance being those which best reflected the mode of life, in connection with the gradual perfection of the different forms in one great ascending series. Modern taxonomists, on the contrary, distrust the more obviously adaptive characters, such as are of least importance from a physiological standpoint being often the most valuable from the phylogenetic point of view. How much Müller was inclined to lay more stress on physiological characters appears from his remark in a letter to Retzius in 1839, when the systematic position of the newly discovered lung-fishes was being discussed: "an animal with a larynx, a trachea, and lungs can be no fish"; yet soon after he had to accept their position.

In 1847 Müller took up the study of a gigantic fossil animal, regarded by some as a lizard (*Basilosaurus*), by others as a sea-snake (*Hydrarchus*), the teeth of which had already been correctly referred by Owen to a marine mammal (*Zeuglodon*). Whilst Müller was busily at work on this perplexing creature, another German zoologist, Burmeister, was able to prove it to have been a Cetacean, the precursor of our modern toothed whales or dolphins. The researches of Müller were published in a folio volume with twenty-seven plates in 1849. In the department of mammalian palæontology he also wrote on the structure of the foot of the edentate *Glyptodon*.

From about this time Müller spent all his holidays at the seaside, to unravel the mysteries of marine inverte-

brate life, a branch of study which has since been so greatly cultivated, thanks to the numerous seaside laboratories which have sprung up within the last forty years.

At the time of his Amphioxus studies, he had been twice to Sweden. In the summers of 1845 and 1846 he went to Heligoland, where he studied the metamorphoses of star-fish and urchins ; late in the autumn of 1848 to Ostend ; early in the following year to Marseilles, where he paid special attention to Holothurians, another type of Echinoderms. During the Easter vacation of 1851 the Echinoderm investigations were resumed at Trieste, and led to one of the most startling discoveries of his whole career, that of the Gastropod mollusc *Entoconcha*, parasitic in the genital gland of the Holothurian *Synapta digitata*, causing its abortion, the first discovered case of parasitic castration. However, he did not from the outset reach the correct interpretation of the facts before him ; the parasite was believed to be an organ of its host ; ideas of alternate generation, the discovery of which had just been made by Steenstrup, and confirmed by Lovén, Sars, and others, however improbable in this case, haunted his mind ; he felt so perplexed, even bewildered, at the mere thought of facts so contrary to the ascertained laws of nature, which, had the interpretation which seemed to him the most obvious been correct, would have upset the very foundations of the zoological system, that, far from experiencing that satisfaction which every new discovery had hitherto brought him, he wished he could have dismissed the whole thing as a nightmare.

As this story of the Entoconcha is one of the great curiosities in the history of Zoology, I shall endeavour to tell it briefly. Holothurians are worm-like Echinoderms with a soft or leathery sac-like skin with calcareous deposits, and an anterior mouth surrounded with tentacles. One of these creatures, *Synapta*

digitata, first discovered on the English coast, was found by Müller in great abundance in the Bay of Mazzia, at Trieste. In his previous work on the Echinoderms, he had given an account of its anatomy from spirit specimens, and de Quatrefages had discovered another species of the same genus to be hermaphrodite. When, at the beginning of the year, great numbers of these Holothurians were procured at Trieste, ova were found in all of them, thus affording confirmation of de Quatrefages's discovery. In summer, when Müller returned to Trieste to continue his investigations, he met for the first time with an individual totally different in its generative organ, and he began to doubt the hermaphroditism of the *Synaptæ*, thinking this to be a male. What was his surprise, however, when, having had large numbers brought to him daily, he found among them an individual in which the anomalous genital organ contained capsules including young snails with spiral shells. This was the commencement of the researches which he prosecuted uninterruptedly for two months, in the course of which he observed sixty-nine times the presence of molluscs or mollusc-eggs in this Holothurian. He soon found out that these aberrant individuals could be easily distinguished externally from the others, the semi-transparency of the body permitting an observer to see whether it contains the normal ovarium or the thick molluscigerous "organ," a sac which nowise resembles that of the ordinary generative organs. One part of the molluscigerous sac contains the male elements, the other the female, in numerous separate capsules. Müller was able to follow out the fecundation and development of the ova, which closely agreed with the same in certain molluscs. The new creature, which he named *Entoconcha mirabilis*, was certainly a snail, with a spiral shell, but, beyond the fact that it was a Gastropod, he could not fix its position in the system.

"That the molluscs *are* developed within the Holothuria," he said in his first communication to the Berlin Academy, which was translated and commented upon by an anonymous writer who was to become one of the greatest zoologists—Huxley, "is clearly made out; how it is possible that they are so developed, I know not. All that I know is the fact, and the mode in which it occurs; and I may further add, that it is impossible the molluscs should have been introduced from without. The Holothurian has not eaten them, for it eats nothing but fine earthy mud, and nothing else is ever found in the intestine; and even if it had, how could they get out of the intestine into the abdominal cavity and the molluscigerous sac? Neither have they crept into the abdominal cavity of the *Synapta*-fragments, for all these are spasmodically contracted at their extremities, so that nothing can either pass from or into tnat cavity with its normally contained saline fluid. Besides, how could a thousand or more molluscs creep in, particularly as they must have entered as yolks? Neither have they crept into the sac from without, since they have arisen from their elements in it. It follows that the sac must either itself be the equivalent of a mollusc, a vermiform metamorphosis of a mollusc as it were, which has made its way into the Holothuria; or it must be an organ of the Holothuria, which instead of Holothuriæ produces molluscs. . . . The whole difficulty, however, does not consist in conceiving the sac to be an animal. A great difficulty for every theory is that the molluscigerous sac is organically connected with the Holothuria. . . . Has this sac, then, perhaps arisen as a bud in the Holothuria, remaining in connection with it, and perhaps having the same relation to the production of the molluscs as the proembryo of certain plants has to their production? Against this view, however, we have the fact that the sac opens at the same place as the ordinary generative

organs of the Holothuria. Perhaps it is a case of the alternation of generations, the Holothuria producing molluscs, from which again Holothuriæ are produced, though it is highly improbable that the alternation of generation ever goes so far ; and besides, the Holothuria has its own peculiar mode of reproduction, its own ova, with whose product, indeed, we are not yet acquainted, but which indubitably is wholly different from a mollusc, and without question is again a *Synapta*."

After suggesting various speculations to which these facts might give rise, Müller did not commit himself to any definite conclusion, but leant towards regarding the case discovered by him as one of "heterogenous generation," that is, the production by a given species of offspring similar to itself, and of offspring dissimilar to itself, by true sexual generation, pointing out that this process is very distinct from the alternation of generations and suggesting that it may explain the mode of introduction of new species upon the surface of our planet, for Müller believed like Cuvier in separate successive creations, as palæontology then seemed to prove.

Such extraordinary suggestions were not likely to pass without challenge on the part of even Müller's greatest admirers, but he himself very soon came round, especially after having discovered, after his return from Trieste, that some specimens of *Synapta*, which he had preserved in spirit, possessed both the normal genital organ and the molluscigerous sac. Six weeks after his first communication, from which we have quoted above, he wrote a modified account for his *Archiv*, in which, whilst still regarding the solution of the problem as impossible, he allowed more weight to the probability of the parasitic nature of the molluscigerous sac, adding "that possibly our mollusc may never be discovered in the adult state, but that,

after a short life as such, it may cast off shell and operculum and change into a parasitic worm, a hermaphrodite mollusc-generator." Now Müller was near the truth, and although he did not himself pursue his researches further, others followed the course which he recommended, that "further investigation must proceed upon the basis of what we know, and explanation must be sought in the common course of Nature."

Writing to Retzius, May 21, 1852, he tells him he is quite upset by this "snail business." Yet he has got so far as to feel certain that the sac is not an organ of the Holothurian, but a separate animal, a view which is adopted in his final memoir on the subject, published late in 1852.

It is now established, by the researches of A. Baur, published in 1864, that *Entoconcha mirabilis* is the larva of a degenerate parasitic Gastropod, so degraded as to consist of a mere tubular sac without nervous system, with just a vestige of intestinal canal and distinct testis and ovary, settling in the genital gland of *Synapta digitata*, which in course of time it absorbs and supplants. As in many degraded creatures, the larva is much more highly organized than the adult, and departs less from the normal pattern of the group to which it pertains.

The question as to how the parasite penetrates into the Holothurian and how its progeny is released has not yet been solved, although from analogy with a related species discovered since, N. R. Harrington (1897) has concluded that the larva is free-swimming and enters the host with the water taken into the respiratory system.

The work done by Müller on the Echinoderms and published between 1840 and his death is considerable. He has investigated the larval conditions of four out of the five orders which constitute this class, and deter-

mined the common plan followed in their development; he has described the anatomy of recent Crinoids, elucidated fossil remains of the same order, and dealt with the systematic arrangement of the Asterids, finally subjecting the organization of the entire class of Echinoderms, both recent and fossil, to a thorough revision. His last paper (March, 1858) was on some fossil Echinoderms from Germany. Shortly before (1857) he had added to our knowledge of the development of the Pteropod molluscs.

We have now reviewed the most salient of Müller's publications. A few words, to conclude, on the man himself during the years of his Berlin professorship. Rather stiff and reserved, impatient of idle talk, and perhaps too much concentrated in his work, as needs must be in a man who, in the space of thirty-five years brought out twenty-four books and about ten times as many memoirs and papers ranging over an enormous field of knowledge, the Professor was inaccessible to ordinary students; but he was an ideal master to his privileged pupils, many of whom have attained high eminence in science. Although rather taciturn at home, he was a most devoted husband and father, sharing with his family the pleasures of his holidays, which, in the latter half of his career, were devoted, as we have seen above, to visits to distant seaside places for the furtherance of his zoological pursuits. Sometimes he would allow one or two of his favourite pupils to accompany him on such expeditions—with fatal results on the last occasion, September, 1855, when the ship which was to bring him home from Norway was wrecked and one of his young companions, a Dr. Schmidt, drowned, Müller fortunately escaping by swimming and clinging to floating wreckage. He never quite recovered from the shock, and never again set foot on a boat.

Müller was no orator, but his lectures were impressive for the thorough mastery he possessed over the subject

of his exposition, aided by graphic demonstrations on the blackboard. For he was an accomplished draughtsman ; the beautiful plates of many of his publications, drawn by himself, testify to his talent. He was also an expert at photography, a rare attainment in those days.

The honour of the rectorate of the Berlin University twice fell on him, a serious burden on the second occasion, in 1848, when his prestige was invaluable during the revolution which had broken out in the capital of Prussia. He himself kept guard over the University buildings opposite the royal palace, and succeeded in protecting them from the devastations of riotous students.

Müller was a vitalist, and remained so to the end of his days, although avoiding controversy on a question on which he was attacked even by two of his most distinguished pupils and friends, du Bois-Reymond and Schwann, and which had been brought to the front by the latter's epoch-making discovery of the cell-theory. Müller felt justly enough that such a question was outside the limitations of experimental physiology ; and, absorbed as he then was in his morphological work, he abstained from taking part in the disputes over the theory of life which sprang up towards the close of his career.

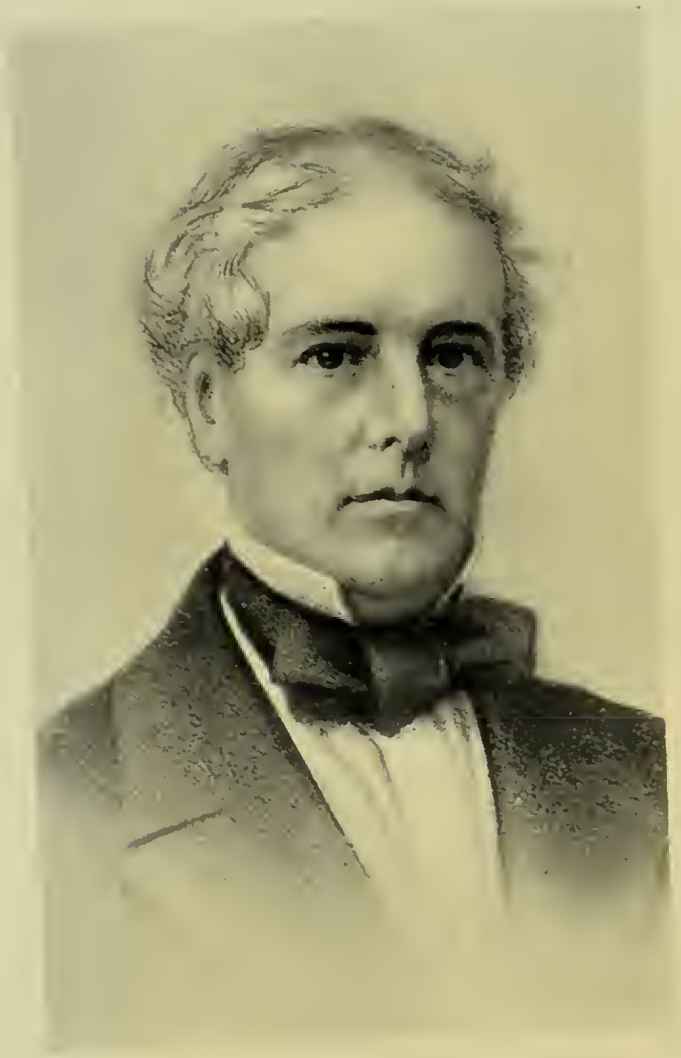
In fact, as he advanced in age and honours, Müller became more and more averse to public controversy, and lost much of the somewhat proud and overbearing manner he had shown, in earlier years, towards some of his confrères. The tone in which, in 1837, he had replied to the attacks of Friedrich Arnold, was for him, in later years, a subject of frequent regret, as he confided to a friend.

Early in his scientific life, Müller had given up philosophical speculation in his writings, the perusal of which often rather conveys materialistic tendencies. His saying, in reference to his own studies, that "nothing

is worth knowing that does not fall under the scalpel" has often been quoted, although it was repudiated by him later in life. Still, he does not seem to have ever dissociated himself from the religion in which he was brought up, and, speaking at his funeral, the episcopal delegate, Provost Pelldram, of St. Hedwig's, his parish priest, afterwards Bishop of Treves, describes him as a man who "both in church and at home served his God faithfully, admiring His wisdom and majesty all the more as he penetrated into the depths of science."

From 1828 to 1856 Müller had enjoyed robust and uninterrupted health. He prided himself on being able to settle down to sleep at any time of the day or night and thus recoup himself from the fatigue of his prodigious work. But towards the end of 1856, after having suffered from insomnia, he was laid up with gastric fever, followed by arthritis in the foot. He recovered, but was never himself again, and as the summer term of 1858 approached, he made up his mind to give up his lectures. He could not discover what was the matter with his health, but he felt so alarmed that he asked his doctor to come and discuss matters with him on April 28th. On the morning of that day, he was found dead in his bed. As he had left instructions forbidding a post-mortem, the cause of his death was not ascertained. His funeral took place on May 2nd, amidst an enormous concourse of representatives of the State, the University, and scientific bodies, and with the prayers of Holy Church. His remains were buried in the Catholic Cemetery in Liedenstrasse.

A statue erected to his memory in his native town was unveiled in October, 1899.



SIR DOMINIC CORRIGAN

SIR DOMINIC CORRIGAN

(1802–1880)

BY

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THE lives of the good and great are as lamps to our feet, to guide and help us on our earthly pilgrimage. Never was a life more illustrative of this fact than that of Sir Dominic John Corrigan. My only fear in attempting to sketch it lies in my own unworthiness. Still, I must make the attempt. So much of my earlier life—from 1854 until Sir Dominic's death in 1880—was spent in close association with him, that I feel not only anxious, but bound, to record the love and gratitude I owe him, and the deep reverence in which I hold his memory.

Dominic John Corrigan was born in Dublin on the 2nd of December 1802. His father, John Corrigan, and his mother, Celia O'Conor Corrigan, lived at that time in a house in Thomas Street, the site of which is now occupied by the church of St. Augustine. They also owned a fee-simple property and cottage called " The Lodge " at Kilmainham. John Corrigan was a man of great intelligence and energy, and, not content to live upon the profits of his farm, he entered into commerce in the sale of agricultural implements, wishing to meet a want he had noticed among the labourers flocking to Dublin on their way to England. His wife was of rare talent and beauty, a cousin of The O'Conor Don of that period, and descendant therefore of an Irish royal race.

John Corrigan prospered in business and was able to give the best possible education to his family. The eldest son, Patrick, went early to America and married there. One of his daughters became a distinguished member of the Loreto Order at Niagara, and two of his grand-daughters became nuns in the Sacred Heart Order in New York. Next to Patrick came Dominic John, the second son. A third son, Robert, who was brilliantly clever, went out to settle in New Orleans, where he died of yellow fever a fortnight after arrival. Of three daughters, Mary and Celia married; the third, Eliza, became a Carmelite nun.

The youthful Dominic was sent as a pupil to the Lay College of Maynooth, at that time a foremost Catholic educational institution in Ireland, where he soon distinguished himself in his studies, and especially by his taste for Physical Science. Owing to this he was constantly employed in assisting the Professor of Natural Philosophy, and became an expert therein. The Professor was the Rev. Cornelius Denvir, afterwards Bishop of Down and Connor, and the life-long devoted friend of his illustrious pupil.

Very early Corrigan made the acquaintance of Dr. O'Kelly, Resident Physician to the College, a very remarkable man, to whom he was subsequently bound apprentice, according to the custom of those days. Dr. O'Kelly was so struck by the ability and industry of the youth that he advised and urged his father to send him to Edinburgh to study medicine. At that time the University of the Scottish capital enjoyed a world-wide reputation, second to none.

His medical studies completed, Corrigan took the degree of Doctor of Medicine at the early age of twenty-three, in the year 1825. The subject of his thesis was "Scrofula." That same year another great Irish physician, the late Dr. William Stokes, also graduated in Edinburgh.

In 1825 the youthful Dr. Corrigan returned to Dublin, commenced his professional life in the house No. 13 Bachelor's Walk, and forthwith devoted himself, with the ability and energy which marked and moulded his entire career, to the study and practice of his profession, and especially to the work of teaching, wherein he rapidly attained a foremost place. In addition to a clearness of perception which amounted almost to intuition, he possessed an extraordinary gift of conveying his knowledge to others. Of this, as one of his pupils, I can speak from experience. I believe it would be impossible for anyone who did not live and struggle for success in the medical profession at the period when Corrigan began, to form an idea of the difficulties which beset a Catholic in the effort to attain it in Dublin. However, as we shall see later on, Corrigan was able to do so brilliantly by his ability and dauntless courage.

Fairly launched in his native city, Corrigan followed up in full earnest the studies he had commenced in Edinburgh, and highly educated as he was at Maynooth Lay College in Latin, Greek, French, and Physical Science, he attended assiduously at Sir Patrick Dun's Hospital the clinical lectures delivered there in Latin by Dr. Toomey, an eminent Dublin physician and professor of the time. Very early in life he was fortunate enough to obtain the post of physician to Jervis Street Hospital, an excellent although somewhat limited field for the exercise of his talents. For this appointment he was obliged, according to the custom of the institution, to contribute a large sum of money to its funds, even though the number of beds at his disposal was only six.

Never was capital better invested, for with careful selection of cases, and devoted attention to them, he laid the foundation of his unsurpassed reputation as a clinical physician, pathologist, and teacher. The two famous essays which demonstrated his genius as an

original observer and pioneer in pathology were written during his tenure of office in Jervis Street Hospital. About that period he occupied in succession the Professorship of Medicine in Digges Street School, in Peter Street School, and later in the Richmond Hospital School, which subsequently was named "The Carmichael" in memory of its illustrious founder. So popular and sought after were his lectures in these Medical Schools, that the great difficulty was to find accommodation for the crowds which flocked to hear them. In 1831 Corrigan was elected Consulting Physician to the famous Catholic College of Maynooth, and held that honourable and most influential position until 1866, when pressure of work obliged him to resign.

In addition, Corrigan filled for some years the post of Visiting Physician to Cork Street Hospital, where he acquired much of that profound knowledge of fever which is exhibited in his famous lectures on that subject, to which I shall refer later. Besides this vast and well-tilled field of clinical study, Corrigan was a diligent reader, and mastered the best literature on medical topics in English, French, and Latin, Morgagni's great work, *De sedibus causisque morborum*, being his chosen favourite. A maxim which he often impressed upon his pupils was—"Choose your one text-book of practical medicine, and let your own clinical records be the commentary to prove its accuracy or otherwise."

The independence of thought thus formulated found expression in his own remarkable original investigations and discoveries. In the year 1832, when just thirty years of age, Corrigan produced his essay on "Permanent Patency of the Aortic Valves." This work—published in the *Edinburgh Medical and Surgical Journal*—as a matter of fact exhausted a difficult and complex subject, leaving no phase unexplained, and brought him great and lasting distinction; so high an authority as Trousseau, whose appreciation of Irish merit was profound,

designating the condition described as the "Maladie de Corrigan." This great compliment has not been forgotten in France, for I, when following the clinique of the celebrated Dr. Henri Bernheim, in the Hôpital Civil at Nancy, in 1890, heard him refer to a case in his ward as an illustration of the "Maladie de Corrigan de Dublin." I may mention here a very interesting incident anent the topic in question. Corrigan travelled a great deal, always visiting the hospitals in his tours. Once, when in Paris, going round the wards with the physician on duty, they came to a patient whose ailment was tabulated as the "Maladie de Corrigan." The doctor, turning to his guest, whose card he had received, asked him if he knew Corrigan of Dublin. "C'est moi, Monsieur," was the prompt reply. Enchanted to find who his guest was, the doctor led him to the lecture theatre and presented him to the class, and a right royal reception was given to the illustrious visitor.

Six years after the appearance of the essay on aortic valve disease, Corrigan produced a second striking treatise on that peculiar form of chronic inflammatory induration of the lung which he named "Cirrhosis." In this paper—published in the *Dublin Quarterly Journal of Medical Science* in 1838—he pointed out clearly the distinction between that disease and tubercular phthisis, with which it had previously often been confounded. In the light of modern pathological investigation his views on this subject become more meritorious than ever, and continue to be regarded as the first step in the right direction, anticipating modern conclusions by fully half a century. Alluding to these two essays, Sir Philip Crampton, in an address delivered at the Royal College of Surgeons of Ireland in 1838, stated that they placed Corrigan in the very foremost rank of pathologists.

The production of these essays, based on personal observation and confirmed by post-mortem examination, at so early an age, demonstrate the high order of

genius which inspired their author. It is not to be wondered at, then, that Corrigan rose rapidly in the estimation of the profession and the public, that practice flowed in to him; and we find that his position entitled him so early as 1834 to move from Bachelor's Walk to the house in Merrion Square West which he occupied as his town residence to the end of his life. Later on, when his vast practice and large income justified it, he built a beautiful seaside residence at Dalkey, some nine miles from Dublin, and there spent each summer. He named it Inniscorrig, and made it a place of such rest as the demands on his time permitted. No one who enjoyed the delightful afternoons and evenings spent there with him and his charming family could ever forget them and their gifted host.

We next find that in 1840—on the death of Dr. John Crampton—he was elected to the post of Physician to the great Hospitals of the House of Industry, the medical portion of which consisted of the Whitworth Medical and Hardwicke Fever Hospitals. Now, at last, he had a field for observation and teaching worthy of his ability, and well he used it. Eight o'clock every morning found him in the wards, recording, explaining, and lecturing upon the cases, followed by a large and most attentive class of students. This practice he carried out for years: in fact, until his necessary pressing engagements rendered it impossible. It would be difficult to say which was most prized—his teaching at the bedside, or his systematic courses of lectures on the practice of medicine. Both—admirably illustrated—attracted round him such a crowd of pupils and young practitioners that the great difficulty was to obtain space in the hospital wards or the lecture theatre.

The Hardwicke Hospital, filled with fever cases of all description and variety, supplied the material for his splendid lectures on these diseases, and the attention devoted to the study of the cases was guarantee for the

accuracy of his views to the minutest details. In 1853, the year before I became a student of medicine, Corrigan published these lectures in book form.

It would be impossible in a short space to give a review of this work, one of the ablest I ever met, and most characteristic of its author. It was not intended as an exhaustive treatise on fever, but rather as a guide to help the student to study that protean malady. Probably no subject presents such difficulty and bewilderment to a beginner in medicine as the febrile state, which enters so largely into most morbid conditions, and in such strange and confusing forms; but anyone who masters Corrigan's lectures on fever is forthwith placed in a position to enter intelligently upon this very complex study. To the last day of life he must persevere in studying and learning more and more about it; but the grand principles of diagnosis and treatment are clearly set forth, in perfect simplicity, in Corrigan's lectures. I can never forget the effect they had upon me, in my third year of pupilage, when I had the good fortune to be selected by Corrigan to take up duty as his resident clinical clerk. It is also to be remembered, to the author's credit, that at a period when typhus, and enteric or typhoid fever, were so often confounded, he saw clearly the fundamental difference between them and laid it down in his lectures. For this achievement he was indebted to his diligence in pathological investigation. The Pathological Society was founded in 1836, and gave Corrigan a matchless opportunity for demonstrating his thorough knowledge of morbid anatomy, and its relation to disease in its various forms. It met weekly during the winter sessions, and seldom was a meeting held without some valuable communication being made by him. Later, in my student days, I was fascinated by these meetings and the sound instruction they imparted. Still later, I often prepared the specimens under his direction, and listened with delight to his comments

upon them. His demonstrations were lucid and explicit, and though his language was the simplest possible, its clearness saved it from the slightest dullness or wearisomeness.

Corrigan was ever a diligent—I may say constant—contributor to medical literature; but I have always held that his masterpieces, in which his special original genius shone, were his essays on disease of the aortic valves, on cirrhosis of the lung, and his lectures on fever.

At the time when I became a student of medicine—in 1854—and first met Corrigan, he was just fifty-two years of age, and had enjoyed for a long period a commanding position and most lucrative practice. His professional income then, and for very many years after, was one of the largest—if not the largest—on record amongst Irish physicians.

It must not be forgotten that at the period of which I write a galaxy of medical talent, in its various departments, existed in Dublin, which made his success all the more difficult and remarkable. Those were the days of Graves, Marsh, Stokes, and Banks; of Crampton, Colles, Carmichael, Cusack, Adams, John Hamilton, Hutton, and Robert William Smith; of Collins, Johnson, M'Clintock, Samuel Gordon, Montgomery, Churchill, Beatty, Evory Kennedy, Frederick Kirkpatrick, and many others. Corrigan was second to none, and, surmounting the disabilities of what was then a down-trodden faith, held the very highest rank.

I can never forget the first day I spoke to Corrigan in the grounds of the Hardwicke Hospital. Of commanding figure, very like Daniel O'Connell, his face beamed with kindness, and his manner, if a trifle brusque, was most fascinating. I put a question to him about a patient we had just seen in the hospital ward, and the painstaking manner in which he explained all I asked established a confidence never after shaken or forgotten. Of his

subsequent kindness to me I could never speak without emotion. If I had been a favourite son he could not have been more partial, and, as I later found, more devoted.

His noble figure, so well delineated in Foley's grand statue in the Royal College of Physicians in Ireland, was such, even then, as fitted his earlier history. He had been an athlete in his way, a splendid horseman, and a famous rider to hounds. His courage and presence of mind were always ready, and I may, in passing, give an illustration.

He was a devoted student of zoology, a generous donor and a constant visitor to the Zoological Gardens in the Phœnix Park. On one occasion while there, a serious accident occurred. A visitor approaching too near the wolf's cage had his hand seized by the savage brute, who held it tight, planting his feet against the bars of the enclosure. Despite the efforts of a policeman standing near, who belaboured the wolf's head with his baton, the enraged animal held his grasp—the blood flowed copiously, and the surrounding crowd were terror-stricken and screaming. Corrigan coolly walked over to the scene, realised the position at once, and saw the remedy. Seizing the policeman's baton, he forced the handle, the narrow end, between the wolf's jaws, and with a sudden twist brought the point against the roof of its mouth. The wolf, in agony, let go the hand and fled to a corner of the cage howling. The poor wounded hand was then bathed and dressed, and the sufferer sent on a car to the Richmond Hospital.

The intimacy, begun at the very commencement of my medical studies, increased year by year; later I became Corrigan's resident pupil, and I wish I could give space here to illustrate the kindness and generosity he displayed towards me.

As a student, and later as a beginner in the practice of my profession, I was closely and constantly associated with him, ever the grateful recipient of his fine teaching

and noble example, up to the day when I knelt by his deathbed in 1880.

We have already seen something of Corrigan's genius and originality, of his power of seeing and explaining what others failed to see or explain ; but in addition he was a man of extraordinary energy and work. How he succeeded in teaching as he did, in carrying on a vast, ever-increasing professional practice, and in lending invaluable help in all the educational, literary, and public work of his time, in truth passes comprehension. Yet, withal, he moved as quietly as if he were only amusing himself.

Besides the three masterpieces alluded to—the article on patency of the aortic valves (in 1832) ; that on cirrhosis of the lung (in 1838) ; and his lectures on fever (in 1853)—he wrote ceaselessly, and never without making valuable additions to medical knowledge.

Even before the article on aortic disease, Corrigan contributed in 1829 a remarkable essay to the *Lancet* on the mechanism of the "Bruit de soufflet et Frémissement Cataire." In 1834 he contributed to the *Cyclopedia of Practical Medicine* the articles on Pemphigus, Plica Polonica, and Rupia. In 1836 he contributed a lucid essay on the "Bruit de cuir neuf" ; and in 1837 a paper on aortitis as a cause of angina pectoris. In 1839 appeared his treatise on the use of remedies in the form of vapour in chest affections—an innovation of treatment which originated the very abundant use of such methods subsequent to his time : in the same year Corrigan wrote on the value of opium in the treatment of acute rheumatism, and in 1841 appeared his admirable article on the diagnosis and treatment of functional diseases of the heart.

In 1846 appeared his famous pamphlet on famine and fever as cause and effect, foretelling the coming epidemic that swept Ireland and so notably reduced her starving population.

In 1860—after a visit to Arcachon near Bordeaux—he delivered a remarkable lecture at the College of Physicians describing its climate, resources, and advantages for patients suffering from pulmonary affections. I was present at that lecture. So great was the benefit of his advocacy of that health resort that one of the avenues there is named after him—"Avenue de Corrigan." In 1861 he visited Greece, and afterwards published a most interesting sketch—"Ten Days in Athens." In 1866 he republished his cholera map of Ireland, originally brought out in 1850.

The foregoing items are only a general, and I fear imperfect, list of Corrigan's ceaseless contributions to medical lore; but I should not omit his valuable advocacy of the use of a small disc of iron, heated to a certain point in a spirit lamp, and used for producing a mild counter-irritation in cases of chronic rheumatism, sciatica, and similar affections. This idea originated with Bretonneau of Tours, but was popularized by Corrigan, and is known as "Corrigan's button."

Sir Dominic's literary activity never flagged, and shortly before his death he produced a startling account of his reminiscences of the early days of dissection in Dublin, before the introduction of Mr. Warburton's Act, which provided that only unclaimed bodies should be used. It so happens that I—in my student days—was intimate with many who had taken active part in the "resurrection" business, and whose narratives confirmed all the horrors of the shocking scenes alluded to in Corrigan's paper.

I believe his last essay was upon Aix-les-Bains, in Savoy, whither I induced him to go in 1875. He derived notable relief from his gouty troubles from that visit, and wrote a most amusing, quaint description of the place and treatment. The foregoing list does not include his numerous addresses at the meetings of various medical societies, even in London, or his celebrated address on

medical education delivered at the meeting of the British Medical Association in Dublin in 1866. That address, and the one on surgery by Dr. Robert William Smith, were the literary gems of that assembly. Dr. Stokes was the President, and I had the honour of being Joint-Secretary with the late Professor Tuffnell.

Notwithstanding the immense labour which his vast practice involved, and his ceaseless diligence in teaching and writing, Corrigan found time to accomplish a large amount of public work. It seems almost impossible to recount it all. He took a deep interest in all educational questions, and was an active Senator of the Queen's University from the first, being elected its Vice-Chancellor in 1871. For twenty-one years he represented it in the Medical Council, and rarely missed a meeting, despite the inconvenience, fatigue, and loss its frequent calls to London involved. He was for many years a member of the Board of National Education, and most punctual in attendance at its meetings. All the medical societies knew him as a regular attendant at their gatherings, and as a most telling speaker. He excelled in debate—clear and incisive, and yet very kindly to opponents. His style was matchless, and his English simple but perfect.

When the question of the introduction of the Vartry water arose Corrigan was its most ardent and influential advocate. The part he took in favouring the project of the introduction of this pure water supply to Dublin was largely instrumental in carrying that most salutary measure. His letter to Dr. Gray (afterwards Sir John Gray), published in the *Irish Times* of 25th August 1860, was exhaustive and unanswerable, and aided powerfully in carrying the day.

It is only those who, like myself, remember Dublin before the Vartry water supply that can appreciate its inestimable value. Up to that time Dublin was supplied by two basins, full of highly contaminated canal water, and with no sufficient pressure to help in extinguishing

fire. I remember well a terrible fire in Westmoreland Street, where a houseful of people were burned alive, no water reaching the higher parts of the house. That tragedy helped not a little in the crisis, and the scheme was carried—not without sharp opposition—and brought a marked improvement in public health. Since then we have had no visitation of cholera, and typhoid fever and zymotic disease in general has greatly diminished. To Corrigan's influence, assisted by that of Sir John Gray, Dublin is largely indebted for this huge measure of reform.

From an early period of his life—in fact, from the appearance of his article on aortic disease—honours poured in upon Corrigan. In 1832 he was elected a member of the Surgical Society. In 1843 he felt a desire to have a surgical diploma, and selected the College of Surgeons of England. On presenting himself before the Board of Examiners, he was asked, "Are you the author of the essay on patency of the aortic valves?" On replying in the affirmative, he was presented with the diploma without further question. In 1847 he was appointed Honorary Physician in Ordinary to Queen Victoria in Ireland, a decoration never before given to a Catholic, which he greatly valued and held to the hour of his death. In 1849 he was made an Honorary M.D. of the University of Dublin.

In 1859 Corrigan was elected to the Fellowship of the College of Physicians of Ireland, and shortly after made President. To this high office he was re-elected no less than five times consecutively, an honour quite unprecedented in that ancient institution; and indeed he proved the wisdom of his electors, for he struck out and perfected the plan by which the present noble building in Kildare Street was brought into existence. Up to that time the meetings of the College were held in one of the large rooms of Sir Patrick Dun's Hospital: a very unsuitable place for such a purpose, and sadly

lacking the dignity of a college. No doubt it was for this reason the College was little known, and its diploma rarely sought. Corrigan proposed that a sufficient sum to erect a suitable college, in a prominent position, should be raised in form of debentures amongst the Fellows, and he commenced by putting down his name for two thousand pounds. Others quickly followed, and the needful sum was promptly obtained, the debentures bearing interest at 5 per cent.

So great and rapid was the popularity of the College under the new conditions and auspices, that the fees from candidates and graduates increased to such an extent as to enable the whole debt to be paid off in a very few years. The Fellows testified their appreciation of Corrigan's great achievement by having his portrait—by Catterson Smith—hung in the examination hall. He himself bestowed a valuable stained-glass window, bearing the arms of the College and the Irish harp and shamrock. Later, by subscription—professional and public—Foley's noble statue was placed in the College, in company with those of Corrigan's illustrious colleagues, Graves, Marsh, and Stokes.

In 1866 Corrigan was created Baronet in recognition of his professional eminence and services to education. In 1874 he was elected Corresponding Member of the Paris Academy of Medicine—an honour only once before bestowed on an Irishman: namely, the celebrated Richard Carmichael.

I have before me now a letter from the famous Professor Andral, of Paris, to Corrigan (dated August 1841), congratulating him upon his "power of observation and deduction from facts." I translate this sentence because, brief as it is, it epitomizes the very special talent of the gifted recipient.

Among many high distinctions, we have seen that he filled the office of Vice-President of the Queen's University, was a member of the Medical Council, and Com-

missioner of National Education. He was also President of the Pathological Society, succeeding Sir Philip Crampton. On retiring in 1866 from the office of Physician to the House of Industry Hospitals, he was elected Consulting Physician, and a member of the Board of the Institution. In 1875 he was elected President of the Pharmaceutical Society. In a word, he held all the positions of honour possible for him to acquire.

During the time that Lord Clarendon and Sir William Somerville were in office, it was well known that Sir Dominic was their trusted friend and adviser. What a record of work, so useful, and so well done!

In 1870 Corrigan entered Parliament as Member for Dublin, in the Liberal interest, and hoping to be of service to the medical profession. He bid fair for success, despite his age; but his health suffered, and his family and friends were glad when he failed to obtain re-election four years later. Be it told, however, to his honour, that his failure was due to his loyalty to the temperance cause and the Sunday closing movement; a policy which raised a storm of opposition against him with a certain party of vast influence in election matters.

The present is not the time or place to speak of Corrigan's personal charm of character. I deal rather with the grander aspects of the great man. Still, his personality was something never to be forgotten by those who knew him intimately. He was loved by his patients, and respected and trusted by his professional brethren. His great kindness, cheerful, encouraging manner, and intuitive knowledge of all that came under his observation were wonderful. He was full also of quiet humour, such as we see in most great men, especially Irishmen. May I quote a single instance? Once, when attending a lady of rank in fever, when he entered her room, accompanied by her anxious husband, he said to the latter: "She is better." The visit completed, when they left the patient, the husband asked him how he knew

at a glance and without examination that the patient was better. "I knew it," said Sir Dominic, "by an infallible symptom—I saw the handle of a looking-glass peeping from under her pillow!" He was right. The lady was better and made an excellent recovery. *Ex uno disce omnes.* Corrigan was ever the same—his greatness never interfered with his observation and appreciation of the smallest detail.

Corrigan had a very high idea of the respect due to the profession, and never permitted the smallest slight. For example:—One bitter winter's day I met him in consultation in a house in our (Merrion) Square. We saw and examined the patient, who was in bed upstairs, and then came down to consult. Having been shown into a comfortless, fireless drawing-room, Corrigan said to me: "I won't consult here," and opened the door into the back drawing-room, where the family were warming themselves over a glorious fire. He said at once: "Let us exchange rooms." The consultation over, we called the family in to hear our fortunately favourable opinion. "Now," said he, smiling, "do you think Dr. Cruise and I could have done justice to the case if we had been left perishing in the other room?" All laughed. It needed Corrigan to do this, yet he was right. The patient's life was at stake; for his sake we were entitled to more consideration. He was most careful never to betray the smallest haste or pressure of time. No matter how really hurried, he never let it be seen, and he taught me never to look at my watch in my consulting room, but to have a clock always visible at a glance, but not prominent. Not alone in great things, but in the smallest, Corrigan was unapproachable.

His idea of the sacredness of knowledge gained through the profession was as exalted as could be, and he carried it to a point for which he sometimes suffered blame—but unjustly. He maintained, and most rightly, that medical men should *never* answer inquiries from Insur-

ance offices. He would say: "Let the companies find out all they can with the help of their skilled medical officers, but never answer their queries, with or without the consent of the insuring party. What we know professionally is sacred, and not for sale." This is absolutely right. His able paper on this subject, read before the Medical Society of the College of Physicians of Ireland, will be found in the *Medical Press and Circular* of 22nd April 1868.

I have already alluded to Corrigan's early struggles against the disabilities which marred his progress, owing to his religion. But his loyalty to the Catholic Church in which he was born, educated, and believed, was unswerving. I am sorry space will not allow me to reproduce here the speech he made at the inauguration of the newly-built Church of St. Augustine in May 1869. That church was specially interesting to him, because it stands upon the site of the house in Thomas Street where Sir Dominic was born in 1802. In that address he described, with all the trenchant power of his eloquence, the baneful influence on Catholics of the unjust and cruel penal laws which still existed in his youth, and the glorious emancipation therefrom which Daniel O'Connell accomplished, happily without the shedding of one drop of blood.

He brought his fine peroration to a conclusion with the expression of earnest hope that absolute religious equality may be established in Ireland, without which there can never be true nationality, and that everyone, whatever his belief may be, shall accept for his motto the words, GOD AND OUR NATIVE LAND.

I heard that speech and can never forget it. During the latter years of his life, despite his magnificent constitution, Sir Dominic's health failed evidently, mainly owing to gout, and about a year before his last illness he had a slight threat of a paralytic seizure. This confined him for a time to his seaside home at Dalkey, where I

attended him; but he recovered sufficiently to resume his practice, and his mental powers never deteriorated, even to the very end. However, on the 30th of December 1879 his left side became paralysed, and he was carried up to bed, which he never after left alive. Sir John Banks and I attended him closely, I sleeping in an adjoining room in his house, in readiness for any emergency.

Surrounded by his devoted loving wife, son, and daughters, and nursed to perfection by experts, Corrigan lingered on for over four weeks. During his last illness he was attended by the Very Rev. Dr. Donnelly, then Administrator of the parish (now the Most Rev. Bishop of Canea). With characteristic simplicity and courage he made his preparation for death, received the last rites of Holy Church, and patient and calm awaited his hour. He spoke of it to me with perfect resignation the day before he died. The end came most peacefully and happily during the first hours of February 1880. For him truly, as St. Augustine says, " the Christian's death is only the dawn of the better life."

The interment took place on 5th February in St. Andrew's Church, Westland Row, after a procession which had not been equalled for years in extent or the rank of those attending. The church was filled by mourners of every creed, while the solemn services were conducted by his brother-in-law, the Most Rev. Dr. Woodlock, Bishop of Ardagh.

The morning following Sir Dominic's death, the *Freeman's Journal* gave an eloquent obituary notice of the great man, from which I extract a paragraph well worthy of the occasion: " For the people of Ireland at large he had a character of which, perhaps, he was not himself altogether conscious. They regarded his career with peculiar interest, and his success with gratified pride; because they saw in him evidence of a Catholic rising against all opposition to the highest position

possible for him to acquire. This feeling was nowise sectarian, it was rather racial and national—they felt that intellectual triumph was their noblest vindication against the contumely which had fallen on them in consequence of the ignorance enforced upon the nation by the penal laws. And certainly no man dared vilify the people who, at such a period, gave such chiefs to the three learned professions as Bishop Doyle, O'Connell, and Dominic Corrigan."

By Sir Dominic's death the medical profession at large lost one of its most conspicuous and distinguished members, the University of Edinburgh one of its most illustrious graduates, and the Irish race one of its finest specimens. Although a thorough Irishman, Sir Dominic was as much at home in London as in Dublin, and while a sincere Catholic in faith, he had too much humanity and breadth of mind to be a bigot. It would be well indeed for Ireland if all her sons possessed such moderation, sense, and good feeling as he habitually displayed in dealing uncompromisingly with delicate and difficult questions.

I must say a few words of Sir Dominic's home life. It was, as those who, like me, were intimate with the family, knew, one of ideal happiness. He married young (in 1829) the daughter of William Woodlock, Esq., a wealthy Dublin merchant. There were three sons and three daughters. The eldest son, John Joseph, was a distinguished officer and captain in the 3rd Dragoon Guards. He died in 1866, leaving an only son, who also died young, the baronetcy then becoming extinct. Sir Dominic's youngest son died in childhood, but the second, William, survived his father a year or so. He was a barrister of distinction. Of the three daughters, the eldest, Mary, married the late Sir Richard Martin, Bart., and died a few years ago. The youngest, Johanna, died in girlhood; and the second, Celia, survived her father only a few months.

ANGELO SECCHI, S.J.

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(1818–1878)

BY

THE REV. A. L. CORTIE, S.J., F.R.A.S.

At the fourth conference of the International Union for Co-operation in Solar Research, held at the solar observatory of Mount Wilson, near Pasadena, California, in September 1910, twelve nationalities were represented by eighty-four delegates, among whom were some of the most distinguished astronomers and physicists in the world. Among the resolutions passed by the Union was one to the effect that the Secchi Memorial Fund which was being raised in Italy should be devoted to the erection of a powerful instrument for solar observations in the land of his birth. Secchi had been dead thirty-two years, and yet his memory is cherished and his fame is propagated by those who are most competent to deliver judgement upon his work in scientific research. His was a career which, in the words of the obituary notice printed in the *Monthly Notices of the Royal Astronomical Society* in the year 1879, "had shed lustre on his country, and had added another to the long list of names of which the Jesuits are so justly proud."

The enemies of Holy Church have made such unwarranted use of science as a weapon of attack against even her most fundamental truths, that an impression has sometimes been produced among many of her children that the pursuit of science is damaging and dangerous to faith. This feeling has been intensified

by the fact that some leading men of science of modern times have been utterly devoid of all religion; while the contrary fact that other leaders in science—such as Pasteur, Chevreul, Röntgen, to mention only a few such names—have been equally remarkable as devoted children of the Catholic Church, is unaccountably forgotten. For a moment's reflection will serve to convince every Catholic mind that, as the Vatican Council declared, "nulla unquam inter fidem et rationem vera dissensio esse potest": there can be no real antagonism between faith and right reason. To which we may append as a corollary an exhortation contained in a letter of our late Holy Father, Pope Leo XIII., to the members of the Scientific Society of Brussels. He thus writes: "Of a truth, when the declared enemies of religion show no weariness, but endeavour ever more and more to proclaim abroad the opposition between science and religion, it is opportune that there should arise on all sides men distinguished for science and piety, who, heartily attached to the doctrines and teaching of the Church, should apply themselves to prove that there can never exist any real opposition between science and religion." We take it that the best mode of proof is by the leading of a holy and unblemished life, and by being a pattern of all the virtues, while at the same time devoted to science. And of this truth the life of Father Secchi is a striking example; for his life is not only the life of one of the leaders of scientific thought of his day, but that of one who knew how to unite religion and science—who was at the same time a great astronomer and a holy priest. His life work, too, was accomplished in the city which is the centre of Catholicism, the Eternal City of Rome, and under the fostering care of its Supreme Pontiff. It is ever so; for Holy Church, far from being opposed to the progress of science, has always been conspicuous for the generous support she has given to scientific studies, even, as in

these later days, in the periods of her greatest poverty. We may take as one instance the re-establishment and the enlargement of the modern Vatican Observatory by our late Holy Father, Pope Leo XIII. Any sketch, therefore, of the life of Father Secchi would be incomplete did we look only to a record of his scientific attainments and ignore the priestly and religious aspect of his character. For it was on the firm foundation of faith and religion that he built up the edifice of science; it was from this source that he drew all his motives for study and research; for his end and object in life was the advancement of God's greater glory. Very appropriately may be applied to him the aphorism of the distinguished philosopher Leibnitz: "I have a high esteem for science, because it gives me the right to demand silence when I speak of religion." The work of Father Secchi in the Church was an apostolate of the demonstration of faith by science.

Angelo Secchi was born on June 29, 1818, at the town of Reggio in Lombardy, situated between Parma and Modena. The chief care of his good parents, James-Anthony Secchi and Louisa Belgieri, was to give their little son the best education which they could command, and accordingly at an early age he was sent to the College of the Society of Jesus which was established in his native town. Under the fostering care of the good fathers the boy advanced in piety and in learning. In his studies he distinguished himself as no mean proficient in Greek literature. But God was calling him to His special service, and accordingly, on the completion of his college course, he entered the novitiate of the Society of Jesus at the early age of fifteen, and remained there two years. Thence he passed to Rome to the famous Roman College of the Society, to add one more illustrious name to the long role of popes, cardinals, bishops, statesmen, warriors, and savants whom it is its pride and glory to have produced. The first course he followed was that

of higher literature, after the completion of which he entered upon his philosophical studies, which extended over a period of three years. Comprised in this course was one on physics and mathematics; and so great was the talent evinced by Secchi in these particular subjects, that, student as he was, he was appointed to assist the professor of these matters at the College of Nobles. After finishing his philosophical studies, he was appointed in the year 1839 to teach a class of grammar in the Roman College, but in the following year was transferred to the professorship of physics and mathematics at the College of Loretto, where he distinguished himself by his concise and clear method of teaching. This post he occupied for four years, when he returned to the Roman College to commence his theological studies preparatory to the priesthood. He was ordained priest on September 12, 1847. He remained in Rome, lecturing at the Roman College, till March 1848, when on account of the revolutionary troubles which had broken out in Italy he was compelled, as a member of the Society of Jesus—which on account of its special work in the education of youth ever bears the brunt of the onslaught of those who would rob the Church of her little ones—to quit his native land with the other members of his order, and fly into exile. With several others he came to England, and was sent to Stonyhurst College, where he devoted himself for a few months to the study of mathematics. But Stonyhurst may claim the honour of having initiated the future great astronomer into the study of the heavens. At that time the observatory, which had been founded in the year 1838, was under the directorship of Father Weld. Besides being a meteorological station, the astronomical outfit comprised a 4-inch equatorial telescope, which is now used as a finder to the 15-inch equatorial which stands in its present state as a memorial to another distinguished Jesuit astronomer, the late Father Perry. This small telescope was then

housed in a circular chamber, surmounted by a cylindrical revolving roof, which rises above the central portion of the meteorological observatory. This was the instrument which Secchi first used, and there was excited in him that enthusiastic devotion to astronomical studies which was the foundation of his subsequent brilliant career. He did not remain long in England, for on October 24, 1848, with twenty-one of his companions in misfortune, he sailed from Liverpool, arriving at New York on November 19. Thence he proceeded to the Georgetown University of the Society of Jesus at Washington, where, while teaching the elements of natural science, he found time to pursue his astronomical studies. The university observatory in which he worked was then directed by Father Curley.

Father Sestini, the assistant of the Roman College observatory, who had also been expelled from Rome at the same time as Secchi, had already arrived at Georgetown, and the two Italian fathers were moreover expecting the arrival of Father De Vico, astronomer and musician, the director of the Roman College observatory. He, however, died in London in 1849, at the early age of forty-three, his death being hastened by the sufferings he had undergone in the revolutionary troubles. Peace having been restored in the same year, Father Secchi was recalled to Rome by the General of the Society of Jesus to succeed Father De Vico in the directorship of the observatory. The observatory had not lacked distinguished directors in its past history; among them Father Clavius, surnamed the Euclid of his age, to whom is due the revision of the calendar which is still in use in all civilized countries, and which bears the name of Pope Gregory XIII., under whose auspices it was undertaken. Nor can we pass over in silence the name of the famous Father Boscovitch, astronomer, philosopher, and poet, whose theories on the constitution of matter received in later days the

support of an eminent English man of science, the late Lord Kelvin. Among the works accomplished by this eminent man we may mention the geodetic measurement of an arc of nearly two degrees on the Appian Road between Rome and Rimini.

Secchi left Georgetown on September 21, and came to England, visiting the Royal Observatory, Greenwich, and thence passed on to Paris, re-establishing the broken communications between the observatories. On his assuming the direction of the observatory of the Roman College in 1849, it commenced a new era of prosperity. The old buildings had for a long time proved inadequate to the needs of modern scientific research, and Secchi determined to commence by rebuilding the observatory and making it worthy of the Eternal City and of the Roman College. As early as 1750 his predecessor in office, Boscovitch, had conceived the design of erecting the observatory on the top of one of the massive piers which support the dome of the Church of Saint Ignatius; but the suppression of the Society of Jesus, and the resulting scattering of its members, had prevented the accomplishment of the design. This design was resuscitated by Secchi, and, aided by the generous contributions of the Holy Father, Pope Pius IX., and of the General of the Society, Father Roothaan, a stately structure was in due course raised above the church dedicated to the founder of the Society of Jesus, in the position selected a century before by Boscovitch. The new observatory was formally opened in 1852, and was speedily equipped for astronomical work by the acquisition of a fine refractor of 9 inches aperture made by Merz of Munich, a large meridian circle by Ertel, and an excellent sidereal clock by Dent. The munificence of the Pope also enabled Secchi to establish in the same place a very complete magnetical observatory. With regard to meteorology, besides the instruments of ordinary type, Secchi invented his celebrated meteorograph, an instrument by which

automatic registrations are made at the same times, and at short intervals of time, of the barometer, the thermometer, the direction and velocity of the wind, and the rainfall. One of these instruments was subsequently exhibited at the Paris Exhibition in 1867, and was esteemed so highly that the Emperor Napoleon III. conferred upon Secchi the decoration of the Legion of Honour, and the Emperor of Brazil gave him the Grand Cordon of the Order of the Rose. But we are anticipating the chronological course of events, for in the very same year that the new observatory was opened, on the 2nd of February, Father Secchi took his final vows, and so bound himself to the life-long service of Almighty God in the Society of Jesus, to promote by every means in his power His greater glory. This was the motive which spurred him on in his astronomical studies, and was the very root of his enthusiasm and devotion to research. Of this we may judge by an extract from his own writings: "To whisper to oneself how magnificent it is to reveal the works of the Creator: this is a stimulus which lasts when all other motives fail. It raises the human intellect above the dryness of mere figures, and produces from such labours a work which is lofty—nay, divine. He who, penetrated with such ideas, contemplates the heavens, has not in his heart the mere oppression of a cold admiration, in considering the depths of space filled with bodies the greater number of which are inaccessible to the most powerful means which Providence has put at the disposition of man, and which, by their immense distances and their prodigious quantity, appear to him but as ill-defined masses of confused light. On the contrary, his heart is filled with a sweet sentiment of joy in thinking of these innumerable worlds, among which each star is a beneficent sun, a minister of God's goodness, spreading life and benefits on other innumerable beings, loaded with the benedictions of the hand of the Most High; thinking too of that privileged order of intelligent beings

to which he himself belongs, and who, from the depths of the heavens, sing a hymn of praise to their Creator." However one might disagree with the implication contained in the above passage, as to the habitability of other worlds besides our own earth, and however modified the scientific conclusions of Secchi with regard to the "ill-defined masses of confused light" may have become under the space-penetrating power of modern giant telescopes, yet all can but admire the sublimity of his ideals, and how his heart and mind recognized his Creator in the works of His omnipotence. As a corollary to these words of Secchi, let me quote the dictum of a modern leader of scientific thought in England, the late Lord Kelvin: "If you think strongly enough you will be forced by science to believe in God, which is the foundation of all religion." How incomparably loftier are such thoughts and ideals than those of materialistic rationalists, whose science is the be-all and end-all of their existence! Let us hear one of the protagonists of this school. The late Professor Tyndall thus addressed an audience at New York: "This then is the cue of the whole matter as regards science. It must be cultivated for its own sake, for the pure love of truth rather than for the applause or profit that it brings." A worthy ideal, but lacking the one element of sublimity which the seeking of God in His creatures entails. And the end? To again quote Professor Tyndall: "Like the streaks of the morning cloud, to melt away into the infinite azure of the past."

There are two main divisions or departments in astronomical science: the older astronomy, or gravitational astronomy as it is sometimes called, which deals with the positions, the motions, the masses, and the distances of the heavenly bodies; and a newer astronomy, which saw its inception with the application of the telescope to the study of the heavens at the beginning of the seventeenth century, and which is concerned

mainly with the physical appearances and constitution of the denizens of the skies. It was to this newer branch of the science, or to astrophysics, that Secchi mainly turned his attention, and it so happened that the period of his directorship of the Roman College observatory coincided with the introduction of a new and powerful engine of research into the domain of the science. This was the spectroscope, an instrument by which the astronomer is enabled to tell the materials to be found in the sun and the stars by an analysis of their light, thus belying the dictum of Auguste Comte, the father of Positivism, who had declared that astronomers might be able to weigh and measure the distances of the heavenly bodies, but that they would never be able to find out what they were made of. The principle of spectrum analysis adumbrated by many men of science, among them by Kepler and Newton, and taught in his lectures by Stokes at Cambridge, was enunciated in 1859 by two German philosophers, Kirchhoff and Bunsen. Secchi was quick to see the immense bearings of the new science on astronomical research, and shares with our own Sir William Huggins the glory of being one of the pioneers of astronomical spectroscopy. Solar and stellar physics was his speciality; in these he was a master, although, as the long list of his published papers testifies, he did not by any means neglect other branches of the science, as is seen by his observations of Mars and Saturn, his rediscovery of Biela's comet on September 6, 1852, his long and patient series of measurements of double stars found in the catalogues of Herschel and Struve, and his detailed descriptions of no less than thirty-one nebulæ. In fact, it is hardly possible to turn to the index of any astronomical book without finding a reference to Secchi.

He was one of that small band of astronomers who laid the very foundations of our present knowledge of solar physics, and laid them well. There was no species of solar phenomenon of which Secchi had not made a

profound study. He executed a long series of observations of sun-spots, their forms, their types, their proper movements, their life-histories, their cyclic changes in number, position, and size. With his spectroscope, in the pellucid Italian sky, he studied the form and extent of the hydrogen atmosphere which surrounds the sun, and the mighty prominences of hydrogen gas that emanate from it. He catalogued their forms, their velocities, as also the constituents of the prominences which contain the heavier metallic vapours. The general spectrum of the solar surface he also studied, and he issued a map of the dark lines indicating the presence of the metallic vapours and the gases that overlie the sun's shining surface or photosphere. He headed two expeditions for the observation of the sun's corona during total solar eclipses. In 1860 he went to Spain for the observation of the total eclipse of July 18. Working in conjunction with Señor Aquilar of Madrid, he obtained four photographs of the corona during the total phase. These, when compared with photographs of Mr. Warren de la Rue taken at another station, set at rest the controversy then rife as to whether the corona was a true solar appendage or was only an optical delusion. Ten years later he observed yet another total solar eclipse at Augusta in Sicily on December 20, 1870.

But Secchi's greatest and most enduring work was the division of stellar spectra into four groups or classes. Of necessity modern researches have considerably augmented Secchi's types, and various rearrangements have been suggested, but even then they form a first and ready term of reference for the greatest number of stars in the heavens. For the purposes of this classification he observed the spectra of no less than 4000 stars. The four great groups are as follows:—Type 1, in which the lines of hydrogen are very marked, as in Sirius, Vega, Altair, Regulus, and Rigel. To this class were assigned about half the stars in the heavens. Type 2,

in which the stars were characterised by numerous fine dark lines in their spectra, as in the case of our own Sun, and in Pollux, Arcturus, Aldebaran, Procyon, and α Ursæ Majoris. In the spectra of stars of Type 3 is a system of nebulous bands, which are more defined towards the violet end of the spectrum. Characteristic of this class is the star α Herculis. Father Sidgreaves has in recent years investigated the spectra of stars, at Stonyhurst, which show a gradual change of spectrum between Secchi's Type 2 and Type 3. Professor Fowler has more recently shown that the series of bands in the spectra of stars of this Type 3 are due to the chemical compound titanium oxide. The long-period variable star Mira Ceti belongs to this class. In Type 4 also a band spectrum is in evidence, but the bands in this case are more definitely defined on their red sides. Many small deep red stars belong to this category. Secchi was the first astronomer to point out the characteristics of these various types of stars, which presumably are arranged in a descending scale of temperature. On this subject he published numerous catalogues and lists of star spectra, the result of many laborious observations. Two of his best-known memoirs dealing with stellar spectra are *Catalogo delle stelle di cui si è determinato lo spettro luminoso,* published at Paris in 1867, and *Sugli spettri Prismatici delle Stelle Fisse,* published at Rome in 1868. Among his regular publications are the *Memoria dell' Observatorio,* supplemented by the *Bulletino meteorologico dell' Observatorio del Collegio Romano.*

In 1854 Secchi was commissioned by the Papal Government to execute the measurement of a geodetic base-line, extending over an arc of two degrees, between Rome and Rimini along the Appian Way, repeating with greater accuracy the measurements made by Boscovitch in 1751. He was also employed by the Papal Government to design and superintend the erection of the lighthouses on the coasts of the States of the

Church, and even the schemes for the water supply of several Roman towns were committed to his skill and judgement. In 1862 he represented his Government in Paris at the International Commission on the Metric System. How he found time to undertake all these labours is difficult to surmise, for he was actively engaged in the direction of his observatory and in its routine work, and, moreover, was a professor of astronomy at the famous Roman College. He delivered also many public lectures on his favourite science, the most noteworthy being that on the Sun, given in Rome during the time of the sessions of the Vatican Council, which was attended by more than three hundred of the fathers of the Council. His memoirs and communications to learned societies number upwards of two hundred, and of these thirty-seven were presented to our own Royal Astronomical Society. Among his larger published works may be mentioned :—(1) *Il Quadro fisico del Sistema Solare*. (2) *L'unità delle forze fisiche*, a work which has been translated into French and German, and has gone through two Italian editions. (3) *Le Soleil*, a popular work embodying his own labours on solar physics, and covering the whole ground of the subject and its cognate branches then known. It reached its second French edition in 1875, and was translated into German. It is a classic on the subject of which it treats. (4) *Le Stelle*, a valuable contribution to a series of popular science volumes, published at Milan in 1878. (5) *Lezioni di Fisica terrestre*, and (6) *Lezioni di Fisica pei Giovanni*, both for the young.

The merits of Secchi's scientific work were soon recognized by the leading academies of the world. It would be useless to rehearse all his scientific honours. We may note, however, that he was elected a foreign member of our own Royal Society in 1856, and an associate of the Royal Astronomical Society in 1853. He was also a member of the French Academy of Sciences,

and of the Imperial Academy of St. Petersburg. In his native land he was one of the Società Italiana de XL., and was for some years President of the Accademia dei Nuovi Lincei.

As we have already seen, the beginnings of Secchi's scientific career were conditioned by the revolutionary movement of 1848, which drove him to Stonyhurst. Its end was to be saddened by the events which led up to the entrance of the Italian troops into Rome on September 20, 1870, since which time the temporal power of the Popes has been in abeyance, and the Vicar of Jesus Christ on earth has become the Prisoner of the Vatican. But Secchi was not destined to share in the decree of proscription which again drove his religious brethren of the Society of Jesus into exile. The observatory of the Roman College had a world-wide reputation, and Secchi was one of the more illustrious Italian men of science of the day, if not the most illustrious. Accordingly, arrangements were made by special Acts of Parliament to retain him in the post which he occupied with so much distinction. He and his assistants were exempt from the decrees of banishment. But his vital powers were slowly ebbing, for the malady which finally killed him was of long duration, and so in the last years of his life he was compelled to abandon active observations, and devoted himself to study, though always directing and controlling the various researches which were undertaken at the observatory by his devoted assistants, chief among whom was Padre Ferrari. In the second week of January 1878 he was compelled to take to his bed, and, though aided by the leading surgeons of the University, science was not able to relieve him. But from this time until his death he seemed to have lost all thought except for the things of God, and his piety, humility, resignation, and patience under suffering edified all who came into contact with him. On January 23 he asked to receive Holy Viaticum, and as

a public profession of his faith he wished to have the Blessed Sacrament carried with all possible ceremony from the parish church. But this was deemed inadvisable, and he received the last rites of the Church privately on the following day. One of the thoughts that consoled him most on his deathbed was that of having spent his life for the cause of religion and Holy Church. "Oh," he exclaimed to one who visited him in his sickness, "if you only knew what comfort I feel in dying, after having spent my life in the service of the Church!"

A religious who is bound by his vow of poverty has no personal property to leave behind him. But it may so happen that he is the legal representative of his order with regard to property that is held in common. Such was the case with Father Secchi with regard to the Roman College observatory, which had been rebuilt and equipped with modern instruments at the expense of the General and Fathers of the Society of Jesus. Accordingly, the observatory was willed by him to his assistant, Father Ferrari. After his death, however, the Italian Government, disregarding these dispositions in his will, annexed the observatory and instruments as part of the confiscated goods of the Jesuits. The director who was appointed to succeed Secchi, though a layman, had been a pupil of Secchi's, and Pietro Tacchini carried on the solar work of the institution with great distinction until the time of his death. More recently Pope Leo XIII. rebuilt and refurnished the old observatory in the Vatican gardens, the present director of which is a Jesuit father, Father Hagen, who was summoned from Georgetown to Washington to take charge of the Papal Institution.

But to return to the death-bed of Secchi. The Roman College, as is well known, was honoured by the holy life and death of a saint in the calendar of Holy Church who is the special patron of youth, St. Aloysius Gonzaga. To this saint Secchi had a great devotion. He there-

fore begged that his Cross of the Legion of Honour might, after his death, be hung as a votive offering on the tomb of the saint, playfully remarking that he had knighted St. Aloysius. The insignia of the Order of the Rose of Brazil he wished to be similarly offered to the holy founder of the Society of Jesus, St. Ignatius of Loyola, He also ardently desired to receive a special blessing from the Holy Father, Pope Pius IX., and his lay-brother assistant, Brother Marchetti, was sent to the Vatican to beg the favour of the Supreme Pontiff. "Willingly do I grant it," said the Pope; adding, "Father Secchi has always known how to unite science with religious virtue, but the two virtues which shone most in him were humility and obedience. We know how often he was urged to accept honours and employments from the Italian Government, but he would never accept them. . . . He always came to us for counsel and advice, and never took a step without first hearing what was our opinion. Truly he has been an excellent religious." Such was the eulogium of the Vicar of Jesus Christ upon earth with regard to this humble son of St. Ignatius and distinguished astronomer.

One more weary month the sick man lingered, expiring on February 26, 1878, at the age of fifty-nine years and three months, his last words being an act of thanksgiving to God for the great favour of dying in the bosom of the Church, a member of the Society of Jesus, and in the Roman College, sanctified by the death of St. Aloysius. He had lived forty-four years of his life as a Jesuit. "Those who instruct others unto justice shall shine as stars for all eternity." The life of Father Secchi is an instruction that makes for justice or holiness by its bright example of humility and obedience, and by its testimony, if testimony were needed, to the truth that science is not the monopoly of materialism and rationalism, but is compatible with deep faith in, and with holiness of living according to, the truths of revealed religion. His

obsequies were celebrated in the Church of St. Ignatius, the body resting in front of the altar between the four columns which support the dome, while the observatory, the scene of his labours, rose as a fitting monument above the poor coffin of the humble religious. In front of the altar — beneath the observatory: altar and observatory sum up the life of Padre Angelo Secchi.

Father Secchi's native town of Reggio in Lombardy has honoured the memory of her distinguished citizen by a tablet in the house in which he was born, by a bust in marble in the museum of the Storia Patria, and by inscribing his name among the illustrious sons of the province.

The resolution passed by the members of the International Union for Co-operation in Solar Research, assembled at Mount Wilson Observatory, California, in 1910, and including delegates from Austria, Canada, France, Germany, Great Britain, Holland, Italy, Russia, Spain, Sweden, Switzerland, and the United States of America, representing the leading astronomical and physical societies and academies of the world, was thus worded: "The Committee expresses the hope that the Secchi Memorial Fund, now being raised in Italy, may be devoted to a tower telescope with spectroheliograph."

JOHANN GREGOR MENDEL

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(1822–1884)

BY

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How many are there who, reading or hearing about "heredity," or "Mendelism," associate these ideas with the name of the Augustinian friar, Gregor Mendel? How many realize that one of the most brilliant and practical discoveries in the history of modern biology is the outcome of some years of careful experiment and patient observation carried out in a monastery garden in Moravia?

If science has a romantic side, surely we have all the elements of romance in the thought of this hitherto little-known friar, a true lover of God and of nature, tending his flowers during the leisure moments of the busy round of a friar's daily life, and meditating the while upon that most elusive of all problems of nature, heredity.

But what, let us inquire, is Mendelism? who was Mendel? and what was the nature of his work?

Mendelism may be briefly described as an experimental method of investigating the laws of heredity, conducted on lines established by the Augustinian friar, Gregor Mendel, between the years 1853 and 1870. The term Mendelism is of recent origin, for Mendel's work remained for some thirty years practically unknown and neglected, until at the beginning of the present century it was brought to light on the Continent by certain biologists who were engaged on observations similar to those undertaken by Mendel. To-day, Mendelism may be considered a distinct branch of biological science, such being the importance of the results obtained by the application of Mendel's discoveries.

Johann Gregor Mendel, to give him his full name, was born in humble circumstances, of Catholic parents, at Heinzendorf in Austrian Silesia, on July 22, 1822. His father, we are told, was a small farmer or peasant proprietor holding his land on tenure by doing agricultural labour for the lord of the place.

In the religious atmosphere which so frequently surrounds homes of this description, and from the early instruction in the truths of the Christian religion received from his parents, the young Johann imbibed those principles of earnest piety and unfailing industry for which he was distinguished throughout his strenuous life. Poor though his parents were, they made great sacrifices to provide their son with as good an education as was at that time available. Mendel's maternal uncle, Anton Schwirtlich, had for some years carried on a small private school in the village of Heinzendorf, there being no Government school until after the death of Schwirtlich.

Mendel was first sent to his uncle's school, and showed so much ability that it was thought worth while to send him, when only eleven years of age, to another school, at Leipnik. He himself seems to have asked to be allowed to study, his ambition having been stirred up by two older boys who went to the same school, and whose acquaintance Mendel had made. At this school he was again noticed for his intelligence and diligence, so that he was soon transferred to the high school and gymnasium at Troppau, where he remained until he had taken the degree known as Licentiate in Humanities. On the completion of his studies at Troppau he spent a year in further study at Olmütz. Being now about the age of twenty-one, the serious question of his future career had to be decided. With the degree gained at Troppau, and the recommendations of his teachers, Johann Mendel could without difficulty have entered upon an academic career in the Government schools; but his thoughts and

ambitions were directed rather towards the religious life. One of his teachers at Troppau was an Augustinian, and hence it was natural that Mendel should choose for his future home the well-known and influential Augustinian monastery at Brünn. Here he would find the solitude and absence of distraction so congenial to his studious temperament.

Picturesquely situated at the confluence of the rivers Schwarza and Zwttawa, near the castle of Spielberg, and surrounded by extensive woods, the Königskloster, as it came to be known later, was founded as far back as the year 1353 by two members of the noble family of Margraves of Moravia, Joannis and Jucundus by name, and was accounted one of the most celebrated in the Augustinian order.

Many privileges were conferred upon the monastery by various popes, and its prior or prelate enjoyed the rank and dignities of a mitred abbot. These privileges were, however, rather the rewards than the causes of renown, for the Augustinians of Brünn for many generations were known for their love of religious observance and the study of letters, and several of the brethren were engaged in more recent times in teaching in the schools of the country, or occupied other official posts, occasionally even being found among the ranks of the legislators.

The monastery to which Johann Mendel sought admission in the year 1843 was indeed no hive of drones. The talents and suitable dispositions of the youthful postulant being already known through his teachers, he was received without delay and given the habit of the order, taking at the same time, according to the usual custom, the religious name of Gregor.

After a year spent in the noviciate in the exercise of the duties of the religious life, Gregor took the vows which were to make him a fully professed member of the Augustinian order, and in due course commenced the study of philosophy and theology in preparation for the

priesthood. During this period, which lasted some eight years, his taste for natural history and keen faculty of observation began to develop. He spent much of his leisure in the monastery garden cultivating various species of plants and following closely every phase of their growth, laying then the foundations of those series of experiments undertaken in later years which have made his name famous in the annals of biology.

The great question of the origin of varieties and species in plants, concerning which so much controversy was soon to arise, seems to have already attracted Mendel's attention.

"From the time of noviciate," writes Professor Bateson, "he began experimental work, introducing various plants into the garden and watching their behaviour under treatment. He was fond of showing these cultures to his friends. Dr von Niessl relates how on one occasion he was taken to see *Ficaria calthæfolia* and *Ficaria ranunculoides* (two forms now regarded as varieties of *Ranunculus Ficaria*) which had for some years been cultivated side by side without manifesting any noticeable change. Mendel jokingly said: 'This much I do see, that nature cannot get on further with species-making in *this* way. There must be something more behind.'"

The scientific bent of young Mendel's mind did not escape the notice of his superiors, who required capable teachers for the many educational establishments under their direction. After his ordination to the priesthood, Mendel was sent, in 1851, to the university at Vienna to study mathematics, physics, and natural science. Here he remained till 1853, and during his stay published a couple of short scientific papers. On leaving the university, Mendel returned to the monastery at Brünn, and was immediately appointed to teach physics in the college, or "Realschule," in this city, a post he occupied until 1868, in which year he was elected by the brethren to be their prior.

As a teacher of science he was specially distinguished for his power of creating enthusiasm for such knowledge among his students, to whom he was devoted. During these happy years of his life Mendel carried out his celebrated experiments of crossing different varieties of edible peas and other plants, to which we shall refer presently.

At this period various opinions were beginning to be discussed concerning the nature and origin of species, and their susceptibility to change, opinions which later took a definite form in the writings of Charles Darwin. While at the university, Mendel's attention was drawn to these opinions; but he was not of the temperament which catches eagerly at the latest scientific hypothesis merely because it affords a plausible explanation of nature's mysteries, and, though acquainted with Darwin's theory, he did not share his opinions. Mendel was none the less interested in the problems which Darwin set about to solve, but the methods adopted by the talented friar were completely different. Darwin observed and collected a multitude of facts, to explain which he propounded an ingenious theory. Mendel went to work to experiment on a large scale, and drew conclusions which were the results and the expression of observations recorded with mathematical precision. These conclusions have again and again been verified, and the method introduced by Mendel has been extended over a large field.

Twelve years elapsed before Mendel made known the results of his experiments. In 1865 he read a paper before the Natural History Society of Brünn entitled "Experiments in Plant Hybridization." This was published in the following year. In 1869 there followed a similar paper dealing with a hybrid *Hieracium.*

Mendel's scientific investigations were not confined to plants: he undertook also a detailed study of the effects of crossing different races of bees, with the view of ascertaining the mode in which the special character-

istics of the several races were inherited. Of these experiments Professor Bateson tells us that "he had fifty hives under observation. He collected queens of all attainable races—European, Egyptian, and American, —and effected numerous crosses between these races, though it is known he had many failures. Attempts were made to induce queens to mate in his room, which he netted in with gauze for the purpose; but it was too small or too dark, and these efforts were unsuccessful."

Unfortunately, no trace of the notes which Mendel made of these important experiments has yet come to light, and it is feared that, during the period of mental depression which overtook him before his death, he may have destroyed them.

In addition to natural history, Mendel was also much interested in the science of meteorology, and devoted considerable time to the observation of sun-spots, and appears to have held the opinion, now accepted by many, that there is a certain connection between the character of the spots on the sun and the condition of the weather.

With his elevation to the prelature of the monastery in 1868, a new and less happy epoch in Mendel's career commenced. In a letter to his friend Nägeli, written after his election, Mendel expressed the hope that he might still find some time to continue his experiments on plant hybridization; but this hope was not destined to be realized. The government of the monastery and other affairs henceforth absorbed his whole attention, leaving him no leisure for his favourite scientific occupations.

Beyond filling the post of president of the Natural History Society of Brünn for a year, his scientific career was closed. Doubtless the Königskloster gained by having such an able prelate, but one cannot but regret the loss which science sustained by the untimely end of a career which promised so much, and which, considering its brief span, has borne such unexpected fruit.

Mendel's business capacity and genius for organization found scope beyond the confines of the monastery. Among other things, he established a fire brigade in his native village of Heinzendorf. In recent years a new fire-station, bearing on its walls a commemorative tablet, has been erected by the inhabitants of the town as a tribute to his memory. For some time Mendel was also chairman of the Brünn branch of the Moravian Loan Bank.

But clouds were gathering on the political horizon, and soon a storm was to burst over the monastic establishments of the country. The persecution of the Church and of the religious orders, which at this time was rife in Germany, extended to Moravia. Heavy and vexatious taxes were imposed upon the monasteries, and the venerable Königskloster, which had rendered such valuable services, was not excepted.

Mendel, however, was not one to yield easily to such injustice, the motive of which was only too apparent. He held out stubbornly against the taxation, refusing to be influenced by the example of other monasteries which deemed it more prudent to yield to the pressure put on them. Strenuous efforts were made to bend Mendel's inflexible will; influential persons visited him, endeavouring, by advantageous offers, to induce him to yield, but it was of no avail. Finally, in 1872, the monastery and its property were sequestrated and converted to municipal uses. It has since become the Landhaus of the city, and the gardens a public park.

Thus once more history has to record how the hand of the spoiler has destroyed an institution in which piety and learning flourished to the welfare of the world in general. Little does the despoiler care for the interests of the community, can he but vent his wrath upon those whose whole offence lies in their devotion to the worship of God and the good of their neighbours. But yet in the long run the people are the losers thereby, not the religious.

To leave thus the ancient cloister and garden in which he had so much interest was indeed a sore trial to Mendel; this, together with the constant conflict with the authorities and the task of reorganizing his community elsewhere, began to react upon his health, and cast a sombre shadow over his naturally joyous temperament. During the last years of his life he suffered much from Bright's disease, to the ravages of which he eventually succumbed on January 6, 1884.

Mendel's claim to scientific celebrity rests upon his discovery of definite laws and sequences which occur in the distribution of the characters exhibited by the offspring of crosses between different varieties of a given species of plants.

The application of Mendel's principles on a wider scale has in recent years thrown much light upon the important but complex problem of heredity.

Before Mendel's time it was well known to plant hybridists that similar types of hybrids constantly reappeared when similar crosses were made, but the reason of this entirely escaped their observation. To quote Mendel's own words: "Those who survey the work done in this department will arrive at the conviction that among all the numerous experiments made, not one has been carried out to such an extent and in such a way as to make it possible to determine the number of different forms under which the offspring of hybrids appear, or to arrange these forms with certainty according to their separate generations, or definitely to ascertain their statistical relations."

Mendel accordingly undertook a long series of experiments on lines calculated to furnish these necessary conditions of success. He chose certain well-defined and constant varieties of the edible pea (*Pisum sativum*), grouping them in pairs according to their distinctive characters. Thus some varieties of pea are very tall,

attaining a height of six feet or more; others are dwarfs of about eighteen inches to two feet in height. These would make a pair. Other varieties have round smooth seeds, or angular wrinkled seeds, which again constitute a pair of contrasting varieties. . . . One member of a pair would then be fertilized with pollen taken from the other: A, for instance, being fertilized by B, and B by A.*

Precautions were first of all taken to remove the stamens from the flowers destined to be fertilized with foreign pollen, because in the pea both stamens and ovaries are found together, and hence self-fertilization takes place.

The results obtained by cross-fertilization between the variety with the round smooth seed and that with the angular and wrinkled seeds were as follows:—

The first crop of seeds obtained were all round and smooth. These were sown the following year, and the flowers of the plants which grew from the seeds were allowed to fertilize themselves and run to seed.

The seed was then carefully collected and examined, when it was found that they were not all alike, some being round and smooth, others angular and wrinkled; the proportion of the former to the latter being roughly 3 : 1. The exact ratio was 2·96 : 1.

Thus the type which remained in abeyance in the first family or generation of seeds reappeared in the second generation.

* *Note.*—The process of fertilization in flowering plants, upon which depends the formation of seeds, and ultimately of a fresh generation of plants, is effected in the following manner. Two kinds of reproductive cells concur in the process, viz. pollen grains and egg cells. The former are contained in receptacles borne at the end of a filament, the stamen; the latter are formed in ovaries. In some plants the stamens and ovaries are borne by one and the same flower; in others, either on different flowers of the same plant, or on different plants. . . . In the pea, for instance, each flower bears both stamens and ovaries. Fertilization consists in the union of a generative cell derived from the pollen grain with the egg-cell formed within in the ovary. When this is carried out between stamens and ovaries of the same flower, the flower is said to be self-fertilized; if between distinct flowers, the process is known as cross-fertilization.

The two varieties of seeds were kept carefully apart and sown in the following year, when Mendel observed that the plants grown from the angular seeds produced only seeds of the same variety. The round seeds behaved otherwise. From some, plants were obtained on which only round seeds were formed; from others, a mixture of round and angular seeds, in the proportion of three round seeds to one angular, was obtained.

Let us represent this by a diagram.

Round. A × B *Angular.*

First family (F_1) A self-fertilized

(F_2) A A A B mixed family

(F_3) A A A A B B

The round variety A exercises a kind of ascendancy or dominance over the angular variety B, so that the first hybrid generation (F_1) produce round seeds. Mendel called the character which appeared in the first generation or family the "*dominant*" character, and *recessive* that which did not appear. Although the seeds which showed the dominant character in the second family were externally all alike, yet their internal constitution differed. One-third bred true, and produced round seeds; two-thirds produced both round and angular seeds. The former are therefore known as "pure dominants," the latter as "impure dominants."

Another important point to notice is that the dominant and recessive characters, which are combined in the first generation, become dissociated or segregated in subsequent generations. This principle of segregation is of greater importance than the principle of dominance, since it manifests itself with greater regularity than the latter, as recent investigations have shown.

Mendel drew from his experiments the conclusion that the germ-cells of the hybrid peas were of different

kinds, some of which bear one, others another character, there being no complete blending of both characters in one germ-cell. It is thus that he explained how the characters which are associated in the hybrid afterwards become separated. Hence it follows that if the germ-cells formed by any given individual are all of one kind, this individual can only transmit this particular character, however mixed its ancestry may have been. So it comes about that the angular seeds B, which are the offspring of the hybrid plant A of the first generation, only produce angular seeds.

Mendel's conclusion was certainly substantiated by the behaviour of the hybrid varieties of peas which he cultivated. In all his experiments he found that certain characters predominated over others, and that a separation of characters temporarily associated in the hybrid occurs in subsequent generations. He found, moreover, that the combination and segregation of characters recurred with almost mathematical precision.

Many curious and interesting experiments on Mendelian lines might be cited, and the remarkable results to which they have led might be detailed. One, at least, may be given without running the risk of overloading this paper with details, and that is the case of the so-called Blue Andalusian fowls, which had for so long baffled the breeders and been their despair.

Every effort to secure anything like a pure strain—that is, a strain which might be relied upon to breed true—was made, and every effort failed. Even when two perfect representatives of the breed were selected as parents the results were almost uniformly the production of what are called "wasters"—that is, offspring which, not reproducing the parental characteristics, were valueless from the point of view of the breeder. Some of these "wasters" were pure black, some were white with black splashes. It must not be supposed that these "wasters" were the sole product of the

breeding. On the average the results of the breeding of a pen of Blue Andalusians has been the production of twenty-five per cent. each of the black and the splashed varieties and fifty per cent. of the blue. Careful examination of this question from the Mendelian standpoint has revealed the fact that the black and the splashed "wasters" are really pure races, and that they behave as such when they are bred. The Blue Andalusian, though it has always been spoken of as "pure," is really a mongrel, and must always remain so; in fact, it has been found that the simplest method of breeding Blue Andalusians is to mate the black and splashed forms together. In this particular case the mongrel does not follow Mendelian laws, for it is a mongrel not resembling either of its parents, and there are no dominants and no recessives. Under these circumstances it becomes impossible to say which of the two parent forms possesses the additional factor.

It is not possible or necessary to follow up this point any further, but it will serve to show the interesting vistas of inquiry opened up by investigations conducted on the lines inaugurated by Mendel. Take the whole question of coloration in flowers, and its cause. At present it would seem as if the colour of a flower were due to two interacting causes or substances. One of these is a perfectly colourless "chromogen" or colour-producing basis. The other is one of those strange substances, so elusive but so important, called "ferments." This ferment exerts its activity upon the chromogen and induces a process of oxidation, and this process leads to the formation of the substance which gives the blossom its exquisite colour. Prolonged experiments on the sweet-pea seem to prove that the colour there depends upon two factors, each of which is independently transmitted according to the ordinary scheme of Mendelian inheritance. How these two bodies exist in the gametes, whether as we now know them in the

flower, or in some other form, is at present an undecided question.

We may now inquire how Mendel's experiments bear upon the problem of heredity. What light do they throw upon this difficult problem? No one disputes the fact that certain characters possessed by individuals can be and are inherited. Mendel was not the first to discern this principle; he was, however, the first who clearly showed how, in certain cases at least, the transmission of parental qualities may be brought about. Moreover, he opened a way to further investigation of the subject. The forerunners of Mendel did indeed suspect the existence of a definite law governing the transmission of hereditary characters, but through imperfect methods of experiment failed to discover the principles which have since become Mendel's title to renown. The logical result of his discovery—its practical application, in other words—lies in the possibility, within certain limits, of controlling the course of hereditary transmission. Applied to plants and animals, it is possible now for horticulturists and breeders to ascertain how new varieties arise, and of what elements they are composed; consequently they are able to propagate such varieties with greater success than heretofore, and weed out when necessary undesirable forms.

One of the most striking applications of Mendelian principles has been the development of a variety of wheat which is not susceptible to the attack of "rust." It was found by experiment that resistance to rust disease was a recessive character, in the Mendelian sense, so that by assiduously cultivating this form a sufficiency of seed was obtained from which wheat was grown which was not attacked by the disease even when growing among or near diseased plants.

With regard to inheritance within the human race, the application of Mendel's principles is more difficult,

for many reasons. Their confirmation has to be sought in the genealogical histories of families in which some obvious and special characteristic is constantly known to appear. From such data it may be ascertained whether the transmission of these characters agrees with what one might expect according to Mendelian principles.

No one would be so rash as to claim that Mendel's views have been received with complete assent on the part of the scientific world. Some have disputed their accuracy at times with almost virulent criticism. Others hold the highest opinion of them, and have not hesitated to claim that Mendel has given the scientific world a key with which many of the secret chambers of nature may be opened. Thus, for example, Mr. Lock, in his very interesting book on *Recent Progress in the Study of Variation, Heredity, and Evolution* (1906), expresses his opinion that "the recent revival of work upon the subject of inheritance by the use of breeding methods has, as a matter of fact, already been rewarded with results as valuable and as clear as could possibly have been anticipated—results which are sufficient in themselves to show that the discovery made by Mendel was of an importance little inferior to those of a Newton or a Dalton."

Of this there can be no doubt, that Mendel's papers, when rediscovered, gave the stimulus to that remarkable outburst of experimentation with regard to inheritance by breeding methods above alluded to; and "thenceforward," says Mr Punnett in the third edition of his useful book on *Mendelism* (1911), "the record has been one of steady progress, and the result of ten years' work has been to establish more and more firmly the fundamental nature of Mendel's discovery. The scheme of inheritance, which he was the first to enunciate, has been found to hold good for such diverse things as height, hairiness, and flower colour and flower form in plants, the shape of pollen grains, and the structure of fruits;

while among animals the coat colour of mammals, the form of the feathers and of the comb in poultry, the waltzing habit of Japanese mice, and eye colour in man are but a few examples of the diversity of characters which all follow the same law of transmission."

Without going into the further details mentioned in the above passage, enough has been said to show the importance of Mendel's work, before which the much-vaunted doctrine of natural selection fades into insignificance. We cannot, however, close this brief account of the illustrious friar without a few remarks concerning the long neglect of his work to which we have already alluded. As we have seen, Mendel's discoveries were first made known in 1865, but failed utterly to attract the notice of biologists till the year 1900, when a confirmation of his laws was published on the Continent by Tschermak, Correns, and De Vries, independently of one another. From this date onwards a host of workers has been engaged in this country and elsewhere in experimenting on Mendelian lines, both with plants and animals, and with conspicuous success.

A prominent Mendelian, Professor Bateson, to whom we owe the introduction of Mendelism into this country, alludes to this neglect in the following words: "This episode in the history of science is not very pleasant to contemplate. There are of course many similar examples, but these must be few in which the discovery so long neglected was at once so significant, so simple, and withal so easy to verify.

"The cause," he continues, "is unquestionably to be found in that neglect of the experimental study of the problem of species which supervened on the general acceptance of the Darwinian doctrines. The problem of species, as Kölreuter, Gaertner, Naudin, Wichura, and the other hybridists conceived it, attracted henceforth no workers. The question, it was imagined, had been answered and the debate ended." This writer adds,

moreover: "Had Mendel's work come into the hands of Darwin, it is not too much to say that the history of the development of evolutionary philosophy would have been very different from that which we have witnessed" (Bateson, *Mendel's Principles of Heredity*, 1909).

Mendel himself was convinced of the importance of his discovery, and is said to have repeated frequently, "Meine Zeit wird schon kommen" ("My time will soon come"); but, failing to arouse the enthusiasm of his friend Nägeli, he does not seem to have made any further effort to spread his views.

The career of this notable priest furnishes another example of the truism that neither the practice of the Catholic faith nor its fuller realization in the religious life is a hindrance to the pursuits of science. Rather may it be said that, guided by the light which the Catholic faith imparts, many occasions of error are avoided. To the Christian man of science it is obvious that, important as science is as regards both the increase of knowledge and the welfare of humanity, it does not embrace the sum total of truth. There exists a higher, a Divine, science to which the science of created things must inevitably lead those who think aright, whose minds are not obscured by passion or prejudice, whose sole desire is to discover the truth wherever it exists, and who realize that passing opinions cannot be the last expression of truth. Truth is one, but sincerity is required to discover it.

LOUIS PASTEUR

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(1822–1895)

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(The writer desires to acknowledge his indebtedness to two excellent biographies—*The Life of Pasteur*, by M. René Vallery-Radot (son-in-law of Pasteur), and the volume entitled *Pasteur*, by Professor and Mrs. Percy Frankland, in "The Century Science Series." From one or other of these works most of the facts hereinafter set forth have been obtained.)

Louis Pasteur was born in 1822 at Dôle, a small town in the east of France. His father, Jean-Joseph Pasteur, was a soldier of the great Napoleon, and fought in the Peninsular War. Later on he married and set up in business as a tanner at Dôle.

He seems to have been a most respectable type of old soldier. His language and manners were not those of a retired sergeant; he never spoke of his campaigns, and never entered a café. On Sundays, wearing a military-looking frock-coat, spotlessly clean, and adorned with the showy ribbon of the Legion of Honour, he used to go out walking on the vine-bordered road leading from Arbois to Besançon. Although far from prosperous, he contrived to give his son, Louis, a liberal education. He sent him up to Paris for a while, and

afterwards caused him to take his degree (Bachelier-ès-Lettres) at the Royal College of Besançon. Pasteur retained through life the liveliest recollections of all his father had done for him, and gave expression to this gratitude in the following noble words, which form the dedication of one of his scientific works :—

"A la mémoire de mon père, ancien militaire sous le Premier Empire, Chevalier de la Légion d'Honneur.
Plus j'ai avancé en âge, mieux j'ai compris ton amitié et la supériorité de ta raison.
Les efforts que j'ai consacrés à ces études, et à celles qui les ont précédées, sont le fruit de tes exemples et de tes conseils.
Voulant honorer ces pieux souvenirs, je dédie cet ouvrage à ta mémoire."

(To the memory of my father, an old soldier of the First Empire, and a Knight of the Legion of Honour.
The longer I have lived, the better I have understood thy warmth of heart and thy strength of mind.
The labour which I have devoted to these studies, as well as to their forerunners, is the fruit of thy example and of thy counsel.
Desirous of honouring these affectionate remembrances, I dedicate this work to thy memory.)

Pasteur graduated at the age of eighteen, and although his father would have been delighted to see his clever son settled down as professor at the local college, his old schoolmaster saw that he was destined for higher things, and urged him to take out special courses of instruction in mathematics and chemistry, so as to qualify for entrance to the great École Normale of Paris—the training school of French scientists and professors. Louis followed this counsel, and went for the entrance examination in 1842, at the age of twenty.

He qualified for admission, but only passed fifteenth out of twenty-two candidates. Most young men would have been satisfied at passing anyhow. Not so Pasteur. Dissatisfied with this performance, he refused admission, took another year's work, went up again, and this time passed in with fourth place. Meanwhile he supported himself by teaching mathematics at a Paris boarding-school, where he gave private lessons from six to seven in the morning. One wonders what a youth of the present generation would think of these hours. "Do not be anxious about my health and work," he wrote home. "I need hardly get up till 5.45; you see, it is not so early."

It was at this period that Pasteur began to feel an enthusiasm for chemistry. In a letter dated 9th December 1842, he writes: "I attend, at the Sorbonne, the lectures of M. Dumas, a celebrated chemist. You cannot imagine what a crowd of people come to these lectures. The room is immense and always quite full. We have to be there half an hour before the time to get a good place, as you would in a theatre; there is also a good deal of applause; there are always six or seven hundred people."

Our young scientist was not the type of man to take for granted any *dictum* laid down by his teachers, however distinguished. He insisted on verifying everything and working out everything for himself. He was assiduous at his attendance at the chemical laboratory, and was in the habit of trying, on his own account, the experiments described, but not actually done, at the lecture. Thus, for example, when the process of making phosphorus was merely explained but not actually carried out, on account of its being so tedious, Pasteur would not rest content. He bought a quantity of bones at the butcher's and set to work. He burnt them, reduced them to a fine ash, treated this with sulphuric acid, and went through all the other stages of the process. The

work took him from four in the morning till nine at night; but what was the labour compared with the joy of possessing sixty grammes of pure phosphorus of his own manufacture!

Endowed by God with such a spirit as this, it was not surprising that Pasteur should be selected by his superiors for promotion. He passed his *Licence* examination, and then the higher one called the *Agrégation* with special distinction in physics and chemistry. This was in 1846. A few months later the Minister of Public Instruction wanted to make him Professor of Physics at a Lycée at Tournon, but his teacher, the eminent Balard (discoverer of bromine), represented that it would be rank folly to bury so promising a talent in the provinces, and succeeded in having the appointment cancelled. Pasteur, left to his laboratory work, at once proceeded to prepare for his final degree of Doctor of Science by undertaking some investigations on the application of crystallography and physics to chemical problems. He prepared a thesis on "The Phenomena relative to the Rotatory Polarization of Liquids," and duly obtained his Doctorate in 1847. His attention became concentrated on the optical properties of certain crystalline substances by virtue of which some of them rotate the plane of polarization to the right, whilst others do so to the left. In 1844 a distinguished German chemist, Mitscherlich, had discovered that the two varieties of tartaric acid possessed different optical properties. (Here I may explain that this is the acid obtained from the crusty deposits called "tartar" in old wine barrels.) The common variety (ordinary tartaric acid) rotates the plane of polarized light to the right, whilst the rarer sort (para-tartaric or racemic acid) possesses no rotatory power. Pasteur, who had been studying crystallography and had acquired skill in the use of the goniometer, was much interested in Mitscherlich's discovery. He prepared a fine set of crystals of tartaric acid and its compounds. He could

not understand why it was that the two varieties, though absolutely identical in the nature and number of their atoms and in their crystalline forms, should yet behave differently towards the beam of polarized light. He subjected his crystals to a more minute and painstaking examination than they had ever undergone previously, and succeeded in discovering on the crystals of the optically active tartrate certain minute faces, which had escaped the attention of the most accomplished crystallographers of the day. He then ascertained that the crystals of the optically inactive tartrate were symmetrical—in other words, when looked at in a mirror they gave an image upon which the crystals could be accurately superposed. The crystals of the optically active acid were dis-symmetrical—their mirror-image was not identical with themselves, but they bore the same relation to it *that the right hand bears to the left.* The next thing Pasteur did was to try various ways of crystallizing the optically inactive acid, and he at last found a method by which he obtained two different sorts of crystals, one sort being the optically active variety, already known, whilst the other set were identical with the mirror-image of these, and had never previously been seen. He at once saw that these latter crystals might possess optical properties that would exactly counterbalance those of the optically active ingredient—in other words, that they ought to rotate the plane of polarization to the *left.* Accordingly he carefully picked out the crystals of the new variety, dissolved them and tested them in the polarimeter, whereupon he found, to his joy, that they turned the plane through just the same angle to the left as the others did to the *right.* Pasteur's acute and perspicacious mind instantly grasped the far-reaching importance of his discovery, and, rushing from the laboratory, overcome with emotion, he encountered his friend Bertrand in the corridor, and, embracing him, poured the good news into his sympathetic ears.

This observation of Pasteur's, made in 1848, was the first glimpse of mankind into molecular architecture—a study which has for its object the discovery not merely of the numbers and kinds of atoms that go to make up a compound, but their actual arrangement in three-dimensional space It is true that the full consequences of Pasteur's work were not foreseen at the time even by himself. The study of organic chemistry was not then sufficiently advanced. Over twenty years had still to elapse before Wislicenus made a similar observation with regard to lactic acid. Le Bel and van t' Hoff were then able to rear the now majestic edifice of stereo-chemistry upon the foundations laid so far back as 1848 by Pasteur. And all this is not merely barren knowledge. It is fraught with consequences the most important to the human race. Thanks to the accurate conceptions of stereo-chemistry, we can now construct new chemical compounds with the precision of an engineer constructing a machine, and for purposes as definite. We can build up new drugs and foretell what their physiological effects will be. We can secure refreshing sleep, reduce fever, and stay the ravages of some of the worst disease-producing parasites, to the attacks of which our human bodies are liable.

Even though all the consequences of this first discovery of Pasteur's were not at the time foreseen, yet, proceeding as it did from a young man of twenty-five, it excited incredulity in the breasts of many senior men, who had worked for long years in the same field, but with less success. Pasteur's paper was referred for report to the greatest living authority, Biot, who sent for the young man and made him repeat the experiment with materials provided by himself (Biot), and under the most stringent conditions. One glance at the optical instrument proved Pasteur in the right, and the illustrious old scientist, who saw the glory of his years of labour thrown into the shade by his young disciple, grasped him by the

hand, and in tones of deep emotion murmured: "My dear boy, I have so loved science all my life through, that this discovery of yours makes my heart throb with joy."

Shortly after this Pasteur lost his mother through apoplexy. He was tenderly attached to her—he could no longer work, but asked for leave of absence, and remained for weeks plunged in grief and incapable of intellectual exertion.

On his return to Paris it was felt that something ought to be done for him, and he was at first sent to teach physics at Dijon. This appointment was not looked upon by Biot and his other scientific friends as sufficiently important for a man of his capacity, and so he was sent to Strasburg (then of course belonging to France) as Professor of Chemistry—a post that suited him well, as the numerous industries of Alsace stood much in need of applied chemistry. Here he became intimate with the family of the President of the College, M. Laurent—an intimacy that proved a turning point in his career, for he proposed to and was accepted by the President's daughter, Mdlle. Marie Laurent. In this important concern Pasteur's insight proved as unerring as it was in other and widely different spheres. The young couple were married on 29th May 1848, and the union proved in every way successful. Madame Pasteur took the deepest interest in her husband's work, shielded him as much as possible from worry, wrote out his daily notes from dictation, and rendered his home life one of unclouded happiness. During the following years everything seemed to smile on him. Three fair children in the home—a sympathetic helpmate to whom he recounted each evening the results of his work during the day, who never scolded him save for not taking enough care of his health, and who was indeed *socia rei humanæ etque divinæ*—such were the elements which, together with acknowledged merit and security from disturbance in his work, made up at this period the life of Pasteur.

To follow his labours during the period of six years which he spent at Strasburg would exceed the limits of this brief memoir. Let it suffice to say that he persevered in his study of dis-symmetrical crystalline compounds, and made the important discovery that right- and left-handed bodies, though chemically identical, are widely different in their behaviour towards living things; so that fermentative organisms growing in a mixture of such bodies will use up only the right-handed, and reject the left-handed tartaric acid. In this way it is possible to separate the optically active compounds. For these researches Pasteur received in 1856 the Rumford Medal of the Royal Society of England—one of the greatest distinctions in the gift of English science.

After spending three years at Lille, as Professor and Principal of the new Faculty of Science, Pasteur was recalled to Paris to take up the post of Director of Scientific Studies at the École Normale. All the time he could spare from lecturing was devoted to a study of the nature of fermentation. The great German scientist, Liebig, persisted in looking upon the processes of decay, decomposition, and fermentation as of purely physico-chemical character. He considered that the beer yeast was as dead as the wort it fermented. In his own words: "Beer-yeast, and in general all animal and vegetable matters in putrefaction, impart to other bodies the state of decomposition in which they are themselves. The movement which, by the disturbed equilibrium, is impressed on their own elements, is communicated also to the elements of bodies in contact with them." In another oft-quoted passage, Liebig says: "Those who attempt to explain the putrefaction of animal substances by the presence of animacules, argue much in the same way as a child who imagines that he can explain the rapidity of the Rhine's flow by attributing it to the violent agitation caused by the numerous water-wheels at Mayence." Pasteur was led by his own researches

boldly to claim for living " animacules " the rôle denied them by Liebig and his followers. He proved, beyond all doubt, that these minute living things oxydized the organic matter in the fluid, and thus caused it to break up into simpler compounds. He discovered a new group of living things called *anærobes*, because they live without air, and showed that they play an important part in the processes of fermentation and putrefaction. At that time, life was thought to be absolutely dependent upon air for its maintenance, and Pasteur's discovery of organisms to which air was a poison, and which could only unfold their activities in its absence, seemed nothing short of a revolution.

Arising out of, and closely allied to, these investigations was the great struggle about " spontaneous generation." The question whether life originates spontaneously had been answered in the affirmative by several well-known writers. Readers of the classics will remember Virgil's description of the way in which a swarm of bees can be made to originate from the rotting carcase of a young bull. Nowadays we smile at the crudity of the idea, whilst marvelling at the beauty of the verse in which the old Roman has enshrined it. A still cruder and more laughable assertion was made by Van Helmont, the Belgian physician and alchemist, who actually supplies a recipe for the spontaneous generation of the domestic mouse. His prescription consists in squeezing some soiled linen into the mouth of a vessel containing grains of wheat, whereupon, after the lapse of about twenty-one days, the wheat will be found to have been transformed into mice—adult ones to boot, with both sexes equally represented!

Such mendacious statements had long been discredited as regards the higher forms of life. But in Pasteur's time many scientists were still to be found who maintained that the minuter forms, such as could only be seen with the microscope, made their appearance spontaneously, that is, without arising from pre-existing germs,

in decomposing organic infusions, dead bodies and the like. About the middle of the eighteenth century the question had been freely debated, the leaders being two clergymen, our own countryman, Needham, on the side of spontaneous generation, or *generatio æquivoca,* as it was then called, and the Italian, Spallanzani, against it. In Pasteur's time the leading advocate of spontaneous generation was Pouchet, Director of the Natural History Museum in Rouen, who came forward with a paper entitled "A Note on Vegetable and Animal Proto-organisms spontaneously generated in Artificial Air and Oxygen Gas." Pasteur entered the lists against this opinion, and devoted four years to a struggle in which he ultimately proved victorious. He took enormous pains with his experiments, and made some useful discoveries *en route,* so to speak, such as the efficiency of a cotton-wool plug in the neck of a flask as a means of preventing the entrance of air germs. He also invented a flask with a long-drawn-out neck, curved downwards like the bent neck of a swan, known to the present day as Pasteur's flask. By this means he showed that, without any plug at all, a putrescible liquid boiled in such a flask would keep good indefinitely owing to the fact that the air coming in deposited its germs on the moist inside of the curved neck, so that they did not gain access to the fluid. He took a trip to the Alps, bringing with him dozens of flasks charged with putrescible fluid, and with their necks (straight ones this time) drawn into sharp points which were hermetically sealed. He climbed the Montanvert, attended by a guide with a mule carrying the case containing the precious vessels. He opened thirteen of them in the inn, where the air was more or less foul and dusty, and sealed them in a few minutes. Next day he brought twenty more to the Mer de Glace, opened them for a short time, and resealed them with a blow-pipe. Nearly all of the first series went bad, whereas of the twenty opened on the Mer de

Glace, only one became altered. In this way Pasteur showed that it is the presence of dust in the air, and not the air itself, that brings about fermentation. In reply to contrary results recorded by Pouchet, Pasteur was always able to show some flaw in the experiment whereby air germs were allowed to obtain access—in one case, for instance, with the mercury used by Pouchet for closing the mouths of his flasks. In the end, the Academy of Sciences decided in his favour by awarding him the prize in a competition for the best experimental work on spontaneous generation. The affair got into the newspapers; the fashionable crowd in Paris, where the Second Empire was then at the zenith of its glory, became interested, and Pasteur was invited to give a popular lecture on the results of his work. All Paris came to hear the serious-looking man, his face full of quiet energy and reflective force. After giving his audience a glimpse of his laboratory methods and results, he concluded as follows: "And, therefore, gentlemen, I would point to that liquid and say to you, I have taken my drop of water from the immensity of creation, and I have taken it full of the elements suitable for the development of inferior beings. And I wait, I watch, I question it, begging it to recommence for me the beautiful spectacle of the first creation. But it is dumb, dumb since these experiments were begun several years ago; it is dumb because I have kept it from the only thing man cannot produce, from the germs that float in the air; from life, for life is a germ and a germ is life. Never will the doctrine of spontaneous generation recover from the mortal blow of this simple experiment. . . . No, there is now no circumstance known in which it can be affirmed that microscopic beings came into the world without germs, without parents similar to themselves. Those who affirm it have been duped by illusions, by ill-conducted experiments spoilt by errors, that they either did not perceive or did not know how to avoid."

By the work thus accomplished, with a different object, Pasteur had laid the foundation of the then unthought-of science of bacteriology. The researches thus outlined constitute a turning point in his career. He never went back to the physico-chemical questions that had engrossed his earlier years, but devoted the rest of his active life to the elucidation of biological problems. The study of fermentation led him to inquire into the causes why that process, as industrially conducted, in the manufacture of wine, for example, of beer, of vinegar, not unfrequently "goes wrong," with the result that sour wine, bad beer and vinegar, more like dirty water than anything else, are produced, to the disappointment and loss of the manufacturer. These undesirable results he found to be due to the intrusion and multiplication of extraneous germs in the fermenting mixtures. He found out that by previously heating to about 55° or 60° C. (130–140° F.), most of these objectionable or "wild" organisms could be killed off and the soil left fallow, so to speak, for the rightful or cultivated ferments, which were thus allowed to do their useful work undisturbed. This process of "Pasteurization" still goes by its discoverer's name, and in this country is chiefly practised in the dairy. By pushing the process still further, he found that all microbic life could be killed off—the process we now know as sterilization.

Ever fruitful in bold generalizations, Pasteur now began to ask himself whether disease in man and the higher animals might not, like the so-called maladies of beer, wine, vinegar, etc., be due to the intrusion of minute organisms which, by setting up processes allied to fermentation in the bodies of their victims, could bring about the disturbances of health that we call disease. His first essay in this field was one of peculiar difficulty. It was the investigation of a malady of silkworms called Pébrine, which had assumed serious proportions in 1865, had pulled down the annual revenue

from this source by over 100 million francs, and reduced hundreds of formerly prosperous silk cultivators to destitution. Here Pasteur found himself confronted with two separate and distinct maladies, often existing in the same *magnanerie* (the name given to a farm where silkworms are raised). One was the real "pébrine," due to a protozoal organism. The other was "flacherie," due to an actively motile bacillus. With infinite pains he succeeded in disentangling the ravelled skein of morbid phenomena, showed how both maladies might be prevented, and thus earned for himself the eternal gratitude not only of his fellow-countrymen, but also of the foreigner. In 1867 he was awarded the grand prize medal of the Exhibition. In 1868 he received the honorary degree of Doctor of Medicine from the University of Bonn, and a prize of 5000 florins offered by the Austrian Government to the discoverer of the best way of preventing the malady of silkworms. In 1869 our own Royal Society elected him one of its foreign members. More recently (in 1896), the town of Alais, the staple industry of which, silk-raising, profited so largely by his exertions, displayed its gratitude by erecting a statue to its benefactor. It shows us Pasteur in the act of attentively examining a sprig of mulberry covered with cocoons. About this time he applied to the Government for a special grant towards a new laboratory, of which he stood badly in need, and was overjoyed when the Emperor took the matter up and caused the minister concerned to set apart 30,000 francs (£1200) for the purpose.

But Pasteur's life became overfull. His results as regards silkworms were called in question, and he had to do a great deal of additional work in order to confirm them. Moreover, he met with severe domestic bereavement, his father and his two little daughters dying about the same time. When he had to start work after his brief seaside holiday in September 1868, he was struck

down with a malady of the gravest kind—paralysis of one side with disturbance of speech. He was most devotedly nursed by his wife, and several of his scientific friends took turns at watching by his bedside. He himself thought that he was going to die, and regretted it; for, as he said, "I should like to have been able to accomplish more for my country." It seemed so hard to die at forty-six in the very midst of his work. To the delight of everyone, his mental powers remained unimpaired, the paralysis gradually relaxed its grip, and he was able to move about once more, though he never fully recovered the use of his limbs.

The Government now decided to confer still further honour on Pasteur by giving him a seat in the Senate. But before this could be done, a catastrophe of appalling suddeness had laid the Second Empire in ruins. The triumphant cohorts of Germany poured like a torrent across the fair land of France. The people turned with fury against the Napoleonic Dynasty, by which they considered themselves as betrayed, and, after the awful convulsions of the Commune, set up the Republican form of government that still holds sway in France. Pasteur was a typical Frenchman, full of the most ardent patriotism. He wished to be enrolled in the *Garde Nationale*, but had to be reminded that a half-paralyzed man is unfit for service. His son went to the war. Pasteur senior tried to go on with his work, but could not. He was overwhelmed by the redoubled calamities that fell upon his unhappy country, and was prevailed upon to quit Paris and retire to Arbois, where he had a little house and vineyard. On learning the news of his country's downfall at Sedan, he took up his pen and wrote the following characteristic letter to his pupil Raulin: "What folly, what blindness there are in the inertia of Austria, Russia, England! What ignorance in our army leaders! We scientists were indeed right when we deplored the poverty of the Depart-

ment of Public Instruction. There lies the real cause of our misfortunes. It is not with impunity that a great nation is allowed to lose its intellectual standard. . . . We are paying the penalty of fifty years' forgetfulness of science, of its conditions of development, of its immense influence on the life of a great people, and of all that might have assisted the diffusion of light. . . . I cannot go on: all this hurts me. I try to put away all such memories, and also the sight of our terrible distress, in which it seems that a desperate resistance is the only hope we have left. I wish that France may fight to her last man, to her last fortress. I wish that the war may be prolonged until the winter, when, the elements aiding us, all these vandals may perish of cold and distress. Every one of my future works will bear on its title-page the words, 'Hatred to Prussia. Revenge! revenge!'"

In such a frame of mind it is not surprising that Pasteur should have cast about for some means of showing the enemies of his country the view he took of themselves and their proceedings. He bethought him of the diploma of M.D., *honoris causâ*, bestowed on him a few years previously by the University of Bonn—a distinction that had given him much pleasure at the time. "Now," he wrote to the Dean of the Faculty of Medicine at Bonn, "the sight of the parchment is hateful to me. I feel insulted at seeing my name, with the designation *virum clarissimum* which you have conferred upon it, placed under the auspices of a name which is henceforth an object of execration to my country—that of *Rex Gulielmus*. Whilst sincerely expressing my profound respect for you, sir, and for the celebrated professors who have affixed their signatures to the decision of your Faculty, I must obey my conscience by asking you to efface my name from your archives, and to take back your diploma as a token of the indignation inspired in a French scientist by the barbarism and hypocrisy of one who, in order to

satisfy his criminal pride, persists in the massacre of two great peoples."

The German's reply was equally characteristic. "Sir—The undersigned, now Principal of the Faculty of Medicine of Bonn, is requested to answer the insult which you have dared to offer to the German nation in the sacred person of its august Emperor, King Wilhelm of Prussia, by sending you the expression of its *entire contempt*.—Maurice Naumann. *P.S.*—Desiring to keep its papers free from taint, the Faculty herewith returns your screed."

Pasteur's rejoinder contained the following passage: "And now, Mr. Principal, after reading over both your letter and mine, I sorrow in my heart to think that men who, like yourself and myself, have spent a lifetime in the pursuit of truth and progress should address each other in such a fashion. This is but one of the results of the character your Emperor has given to this war. You speak to me of *taint*. Mr. Principal, you may be assured that taint will rest until far distant ages on the memory of those who began the bombardment of Paris, when capitulation by famine was inevitable, and who continued this act of savagery after it had become evident to all men that it would not advance by one hour the surrender of the heroic city."

Like his grandfather, Pasteur's son served in the army as a non-commissioned officer, and, as there was no news of him, his father, accompanied by Madame Pasteur and their daughter, set out to look for the boy amongst the scattered remnants of the Eastern Army Corps. After a search as anxious as it was hazardous, they came across the young man and took him across the frontier into Switzerland, where he recovered from an illness due to fatigue and privation. He afterwards rejoined his regiment. Whilst awaiting the moment when he could resume his scientific activities, Pasteur reflected deeply on the causes that had brought about what seemed at the time to be the irretrievable downfall of his beloved

country. "The victim of her political instability," he wrote, "France had done nothing to keep up, to propagate, and to develop the progress of science in our land; she has lived on the past, thinking herself great by the scientific discoveries to which she owed her material prosperity, but not perceiving that she was allowing the springs of those discoveries to become dry, whilst neighbouring nations were diverting them to their own benefit and rendering them fruitful by their work, their efforts, and their sacrifices. Whilst Germany was multiplying her universities, establishing between them the most salutary emulation, bestowing honour and consideration on the masters and doctors, creating vast laboratories amply supplied with the most perfect instruments, France, enervated by revolutions, ever seeking for the best form of government, was giving but scanty attention to her establishments for higher education.

"The cultivation of science in its highest expression is perhaps even more necessary to the moral condition than to the material prosperity of a nation.

"Great discoveries—the manifestation of thought in art, in science, and in letters—in a word, the disinterested exercise of the mind in every direction and the centre of instruction from which it radiates, introduce into the whole of society that philosophical or scientific spirit, that spirit of discernment, which submits everything to severe reasoning, condemns ignorance, and scatters errors and prejudices. They raise the intellectual level and the moral sense, and, through them, the Divine idea itself is spread abroad and intensified."

Melancholy as were Pasteur's reflections at this sad crisis in his country's fortunes, they would have been incomparably more bitter still had he been aware that, if the consequences of his own researches had been as well understood in France as they were abroad, the lives of many thousands of gallant Frenchmen then dying of

their wounds received on the field of battle might have been preserved. Germ-borne diseases, as deadly as they were preventible—suppuration, blood-poisoning, erysipelas, gangrene—were rampant in the French military hospitals, and proved to be veritable scourges which the surgeons, not realizing their true nature, confessed themselves equally unable to cure and to avert. And yet three years had already elapsed since the brilliant young Edinburgh surgeon, Dr. (now Lord) Lister, had laid down those methods of antiseptic treatment which, long years after, he ascribed in the following noble words to the teachings of Pasteur. "Truly," said Lister, addressing Pasteur on the occasion of his jubilee celebration, "there does not exist in the entire world any individual to whom the medical sciences owe more than they do to you. Your researches on fermentation have thrown a powerful beam which has lightened the baleful darkness of surgery, and has transformed the treatment of wounds from a matter of uncertain, and too often dangerous, empiricism into a scientific art of sure beneficence. Thanks to you, surgery has undergone a complete revolution, which has deprived it of its terrors and has extended almost without limit its efficacious power."

Shortly after the war, Pasteur, although not a doctor, was elected a member of the Academy of Medicine. He visited the hospitals and noticed how wounds were bandaged without being properly cleaned, so that foul-smelling pus accumulated in them, and soon produced general blood-poisoning. By the aid of the microscope he pointed out to the half-incredulous surgeons the micro-organisms swarming in the purulent matter, and entered into details as to the precautions necessary to get rid of the germs present in the wound and in the dressings. He prescribed that all instruments should be passed through a flame, and that all the dressings should be heated to a very high temperature, in order to destroy the germs.

The old wrong ideas did not die without a struggle. There were not wanting doctors who resented the intrusion of a mere laboratory scientist, and a non-medical man to boot, into what they regarded as purely professional matters. They scoffed at Pasteur's bacteria and at "laboratory surgery," which, as one of them, Dr. Chassaignac, said, "has destroyed very many animals and saved very few human beings." "Everything," he went on to say, "cannot be resolved into a question of bacteria!" And then, sarcastically, little thinking how near the truth he was, "Typhoid fever, bacterization! Hospital miasma, bacterization!"

Despite these scoffers, Pasteur's ideas, proved as they were up to the hilt by conclusive experiments, carried the day. His merit was now universally recognized, and a bill was introduced into the French Parliament to bestow upon him some substantial token of his country's gratitude. The Government suggested a life annuity of 12,000 francs (£480). "The amount," said the introducer, "is indeed small when compared with the value of the services rendered, . . . but the economic and hygienic results of M. Pasteur's discoveries will presently become so considerable, that the French nation will desire to increase later on its testimony of gratitude towards him, and towards science, of which he is one of the most glorious representatives." Half the amount of the annuity was to go to Pasteur's widow. The bill was passed by 532 votes against 24.

It was at this moment of triumph that Pasteur's attention became definitely concentrated on the nature and causation of disease. By a remarkable, perhaps an unprecedented, transition, the man who had begun by studying the nature and properties of crystals, who had then probed to the bottom the chemical mysteries of the brewer's vat and wine cask, who had spent years in combating the doctrine of spontaneous generation, now found himself engrossed by the problems of infectious

disease. Arrived at a period of life (fifty-five) when many men are thinking of retiring from active labours, Pasteur plunged with enthusiasm into the investigation of questions that lay altogether outside his real province, which was that of chemistry and physics. The efficiency and resourcefulness of his experimental work in a department for which his early training might be said—had he been an ordinary man—to have absolutely unfitted him, are certainly calculated to evoke our wonder and admiration.

The first maladies that attracted his attention were those affecting domestic animals, and thus inflicting injury on the agricultural industries of his dear native land. For years a mysterious disease called *charbon*, or splenic fever, had been making havoc among the sheep in the pastoral provinces of Beauce, Brie, Burgundy, and Auvergne. Sheep so stricken often died within a few hours: they drooped their heads, gasped for breath, blood-stained fluid came from their mouths and noses, they fell and died, whilst after death the least cut on the swollen carcase gave issue to black and viscid blood—hence the name "anthrax" (Greek for coal). Nor was the disease confined to sheep. Oxen and horses also suffered, and even man did not escape. Butchers or shepherds who incautiously soiled their hands with blood of the dead animals were often attacked with a hideous swelling called "malignant pustule," which, unless thoroughly cauterized or excised, rapidly produced fatal blood-poisoning.

About the year 1850, Davaine and also Rayer had put under the microscope drops of blood taken from the dying animals, and had seen little transparent motionless rod-like bodies, which were not present in the blood of healthy animals. Their discovery passed unheeded at the time. About the time when Pasteur was thinking of taking up the subject (1876), Robert Koch, then a young country practitioner in a small town of East

Germany, announced that he had succeeded in growing the little rods in the aqueous humour of an ox's eye, had transplanted them from one drop to another, had seen them grow out into long tangled filaments, and form spores somewhat after the manner in which peas form in the pod. Koch likewise showed that by injecting the anthrax rods or bacilli, as they are now called, into guinea-pigs, the disease could be reproduced. The same effect was also produced by feeding animals with material containing the spores. To all these researches it was objected that not the bacilli but some other material derived from the infected animal was the true cause—the bacilli, said the objectors, are only its accompaniment. This view Pasteur successfully refuted. He prepared a series of flasks containing sterilized nutrient broth, and introduced into the first of them, with the usual precautions against accidental contamination, a minute trace of blood from the infected animal. He set it aside in a warm chamber to develop, and next day saw it quite cloudy with flakes consisting of long chains of bacilli. From this turbid liquid he transferred the minutest trace to a second flask, got the same result, and so on from day to day, till, say, ten or twenty flasks had been so inoculated. He then tried the effect of injecting into an animal a few drops of the liquid contained in the last of the flasks, and found that it succumbed to anthrax. The dilution to which the original droplet of blood had been subjected was so inconceivably great that the disease must clearly be attributed to the only thing derived from that blood that persists through the whole series of the flasks, namely, the bacilli : no matter how many flasks were used, five, twenty, or a hundred, the result was always the same. The bacillus and nothing but the bacillus was responsible for the disease.

Further objections were raised. It was pointed out that the blood of animals that never had anthrax, but had been choked or felled and allowed to lie on the ground

for a day or two, contained bacteria like those of anthrax, and would cause death, if inoculated.

Pasteur showed that the blood of such animals owed its virulence to another disease germ, superficially resembling that of anthrax, but differing from it in being motile and in its inability to grow in the presence of air. This germ he called *Vibrion Septique.* We now know it under the name, bestowed by Koch, of the *bacillus of malignant œdema.*

The next subject of Pasteur's researches was chicken-cholera. In the blood of the affected fowls he soon discovered what he termed little specks of extreme minuteness. He had some difficulty in getting them to grow outside the body, but at last succeeded in devising a medium that suited them—a broth made of chicken gristle, neutralized with potash and sterilized at a temperature of 110° to 115° C. The smallest drop of such a culture given to a chicken on a few bread crumbs was sufficient to set up the disease. A chance observation made by Pasteur while studying this malady proved to have momentous consequences. During his absence on vacation his cultures were not renewed. On his return, he found that these old cultures had become incapable of causing the disease in its fatal form. Fowls inoculated with them became ill, but recovered. These same fowls, on being injected shortly afterwards with fresh cultures of proved virulence, remained unaffected. Pondering over this, Pasteur began to ask himself whether some reliable way could not be found of so modifying a virulent germ as to convert it into a harmless *vaccine*, inoculation with which would protect the animal from the naturally acquired disease. Accordingly, Pasteur set about experimenting, and at last hit upon the plan of forcing the anthrax germ to grow at a temperature higher than that to which it was accustomed. He found that when cultivated at 108° F. instead of 98°, it soon lost its property of forming spores, and, moreover,

when inoculated, failed to kill, but only gave a mild attack which protected against the virulent bacillus. It was on the 28th of February 1881 that Pasteur came forward at the Academie des Sciences with his memorable paper on the Vaccine of Splenic Fever. He showed how the degree of virulence could be exactly graduated, and how it was possible to restore to the modified or "attenuated" bacillus its primitive deadliness.

As usual, his conclusions were at first doubted, his facts were called in question. The editor of one of the veterinary journals, a M. Rossignol, wrote an ironical article poking fun at him. "Microbiolatry," he wrote, "is the fashion; it is a doctrine that must not even be discussed. Henceforth, the germ theory must have precedence of clinical observation. The microbe alone is true, and Pasteur is its prophet." Confident that Pasteur's theories would break down under a practical test on a large scale, Rossignol began an anti-microbe campaign and collected money to procure animals for a test. The programme was drawn up and left Pasteur no loophole of retreat. Sixty sheep were to be procured by the Melun Agricultural Society. Twenty-five of these were to be vaccinated by two inoculations at twelve or fifteen days' interval. Some days later, these twenty-five, and also twenty-five others, were to be inoculated with anthrax culture of high virulence. "The twenty-five unvaccinated sheep will all perish," wrote Pasteur; "the twenty-five vaccinated ones will all survive." These latter were to be compared afterwards with the ten sheep that had undergone no treatment in order to show that the vaccination itself did no harm. There were other conditions which made the test still more stringent, and Pasteur's friends felt uneasy lest he might have committed himself too deeply. "If he succeeds," wrote the veterinary press, "he will have endowed this country with a great benefit, and his adversaries must prepare to follow, chained and prostrate, the chariot of

the immortal victor. But he must succeed: such is the price of triumph. Let M. Pasteur not forget that the Tarpeian Rock is near the Capitol."

The experiment was duly carried out in the presence of an immense crowd of witnesses, comprising the civil authorities, delegates from agricultural, medical, and veterinary societies, as well as many journalists. It proved a complete success. Pasteur had a sleepless night owing to some of the vaccinated animals showing a sharp rise of temperature. It was no wonder, considering that they had received a threefold fatal dose! In the event they all recovered, and on the final day the carcases of twenty-two of the unvaccinated sheep were lying side by side in the farmyard, two others were expiring with the characteristic symptoms of splenic fever,[1] whilst all the vaccinated sheep were in perfect health.

Pasteur's triumph was now complete and unquestionable. He found himself the most famous man in France. The Government offered him the Grand Cordon of the Legion of Honour. Pasteur would only accept it on one condition: the red ribbon for his two fellow-workers, Doctors Roux and Chamberland. "What I chiefly wish," wrote he, "is that the discovery should be consecrated by an exceptional distinction to two devoted young men." His condition was at once acceded to, and he and his assistants received the coveted decorations.

Passing over many researches, some more, some less successful, that occupied the last two decades of Pasteur's well-filled life, we will conclude with a brief account of his work on hydrophobia, or rabies. The mystery in which this horrible disease was enshrouded had haunted Pasteur's mind for many years. In 1880 he took up work on it, his first material being provided by two mad dogs sent into his laboratory by M. Bourrel, an old army veterinarian, who had long been searching for a remedy

[1] The twenty-fifth unvaccinated sheep died of anthrax a few days later.

for this most justly dreaded of all maladies. One of these dogs had the form known as dumb madness: his jaw hung half opened and paralyzed, his tongue was covered with foam, his eyes full of wistful anguish; the other, suffering from the ordinary or furious madness, made savage darts at anything held out to him, with a rabid glare in his bloodshot eyes, and gave vent to despairing howls. Shortly afterwards, Pasteur learnt from Professor Lannelongue that a child of five years, bitten on the face a month previously, had just been admitted into the Hôpital Trousseau. Pasteur, overmastering his repugnance to the sight of pain, went to see the poor little patient, who presented all the characteristic symptoms—spasms and convulsions, ardent thirst, combined with impossibility of swallowing. After nearly twenty-four hours of agonizing torture, the child died—suffocated by the mucus that filled its mouth. Pasteur's first step was to take from the child a specimen of the saliva, in which the virus of the disease was supposed to be present, and inoculate it into rabbits. They died in less than two days. He examined their blood and found in it a microbe which he isolated and studied. It proved highly virulent; but was it the genuine microbe of hydrophobia? The symptoms it gave rise to were more like those of ordinary blood-poisoning, and the incubation period was much too short. Pasteur cautiously abstained from drawing any conclusion. "I am absolutely ignorant," said he at the Academy of Medicine, "of the connection there may be between this new disease and hydrophobia." He then tried experiments with saliva from persons suffering from other maladies, and even from healthy adults, and often obtained the new disease, "sputum-septicæmia," as it is now called. From this it was clear that he was "on the wrong track"—he had discovered a hitherto unknown disease germ, but not that of hydrophobia. Without being discouraged, Pasteur continued his in-

vestigations, at no small risk to himself and his helpers, as may be gathered from the description given by M. Vallery-Radot of what used to take place. "One day, Pasteur, wishing to obtain a little saliva direct from the jaws of a rabid dog, two of Bourrel's assistants undertook to drag a mad bulldog, foaming at the mouth, from its cage; they seized it with a lasso and stretched it on the table. These two men, thus associated with Pasteur in the same danger, with the same calm heroism held the struggling, ferocious animal down with their powerful hands, whilst the scientist, by means of a glass tube held between his lips, drew a few drops of the deadly saliva." They ran a terrible risk. It was all no use. No satisfactory results could be obtained from the saliva. Pasteur then tried the blood, but also in vain. As his experience grew, he gradually became convinced that the true seat of the virus lay in the central nervous system. He tried inoculating with the *medulla oblongata* (part of the brain) taken from a rabid animal, and succeeded in reproducing the disease. At first the injections were made under the skin. This method did not yield uniform results, and so he tried placing the virus directly on the brain. This was accomplished by the operation called trephining, which means that the animal was chloroformed, and a small round piece cut out of its skull. The inoculation was then made directly into the brain, the wound closed up, and the animal allowed to recover. With constant practice this operation came to be performed with such speed and skill that the animal, on regaining consciousness, seemed the same as usual. But after the lapse of about a fortnight it invariably developed hydrophobia and died. The seat of the microbe had now been discovered. This was a great step in advance. But it was followed by as great a disappointment. The microscope revealed no bacillus. The culture-flasks, abundantly inoculated with rabid brain-matter, yielded no growth. The microbe could

not be found. (Now, after the lapse of thirty years, we know why it was that Pasteur could not see this microbe, or grow it.) Not being able to cultivate the virus outside the body, Pasteur adopted the only means open to him—he grew it in the brain of living rabbits, transferring it from one that had just died to fresh animals. He noticed that the time that elapsed between inoculation and the outbreak of the symptoms gradually became shorter, until it was at last reduced to seven days. Pasteur called this seven-day virus, *virus fixe,* because it took a fixed time to produce the disease, instead of the variable and inconstant periods taken by the virus procured from ordinary mad dogs as captured in the streets.

This important progress made, Pasteur began to see his way to the final goal—that of immunization. Having found out how to intensify the virus, he now sought for a means of weakening it. The simplest possible plan was that which he had learnt by accident in the case of chicken-cholera, namely, the lapse of time. Accordingly he took the spinal cords of rabbits that had just died of *virus fixe,* suspended them by threads in flasks, the air in which was kept dry by a lump of caustic potash lying at the bottom, and kept them at a constant temperature for days. At intervals he tried the effect of inoculating rabbits with matter from these cords, and found that after a fortnight in the flask all virulence was lost. The shorter the period of drying, the greater the amount of virulence that was retained. Accordingly, Pasteur next proceeded to an immunization experiment. Into a number of dogs he injected first of all some spinal cord that had been kept a fortnight, next day some that had been kept thirteen days, and so on till he was giving them material from rabbits that had only died that very day—the redoubtable *virus fixe.* The dogs so treated remained well, and it was found that they could be bitten by mad companions or even intra-cranially inoculated

without contracting the disease. They were absolutely immune against hydrophobia. All this work was carefully watched by a scientific commission specially appointed by the government for the purpose. A place in the park of Villeneuve l'Etang near St. Cloud was set apart for the numerous experimental animals with their kennels and cages. Pasteur was no surgeon, and never operated with his own hands. The difficult operations rendered necessary for these investigations were at first performed by Dr. Roux, and later on by skilled laboratory porters. During the years 1884 and 1885 the work went steadily on. The immediate object was no longer to render a dog immune to rabies *before* being bitten, but to prevent it from acquiring the disease by treatment begun *after* it had been bitten or otherwise inoculated with the virus. This was also successfully accomplished. The path was now at last opened straight to the ultimate goal—the rescue of a human being from this most dreadful of diseases.

In July 1885 a suitable case presented itself in the person of a little Alsatian peasant-boy, Joseph Meister, who, on his way to school, had been knocked down and terribly bitten by a furious dog, pronounced rabid by the veterinary surgeon. The wounds (fourteen in number) had not been cauterized till twelve hours afterwards, and then only with carbolic acid. In the opinion of all, the poor little fellow was doomed to the most agonizing of all deaths. Pasteur felt and expressed the deepest anxiety as to the advisability of trying his new method of immunization on a human being. He consulted the eminent nerve specialist, Vulpian, who examined the boy himself and decided, in conjunction with Dr. Grancher, one of Pasteur's collaborators, that not a moment was to be lost. They started, therefore, by injecting material fourteen days old and quite devoid of virulence. The little boy, who had been screaming with terror beforehand, soon dried his tears on finding that he had only

to suffer a slight pin prick. As days passed on, and Pasteur found himself doing a thing never before done in the history of the human race—deliberately administering to an innocent child the virus of the most deadly of all known diseases—his anxiety became terrible. He had a series of sleepless nights. But there was no drawing back now. Each day a number of fresh rabbits were inoculated with the cords used for the boy, so as to test their virulence. At last, on the twelfth day, he was treated with the dreaded *virus fixe.* At the same time, some of the same cord was given to a number of rabbits, all of which developed hydrophobia on the seventh day. *The boy remained well.* It was the surest test of the successful immunity conferred on the patient. Pasteur had conquered the terrors of hydrophobia!

When the news was spread, people who had been bitten by rabid dogs began to pour in from all sides, foreigners as well as Frenchmen. Doctors came also, desirous of studying the method. The "service" of hydrophobia became the principal business of the day. Everything was done systematically. Names, dates when bitten, history of the patient, and *post-mortem* examination of the dog were all entered up, and patients were carefully classified, so as to avoid any possibility of receiving a wrong virus, which might prove fatal. There was an occasional failure. A girl, aged ten, who had been severely bitten on the head *thirty-seven days beforehand,* was brought up. Pasteur looked on the case as hopeless, on account of the time that had elapsed since the bite, but allowed himself to be persuaded to give the treatment. The child returned to school, but shortly afterwards was seized with breathlessness and convulsions. She could swallow nothing. Pasteur sat by her deathbed, and as he went down the staircase, burst into tears.

Despite occasional failures, which were mostly due to the treatment having been begun too late, the success

of the Pasteur method was phenomenal. Out of his first 350 patients only one succumbed, the little girl just mentioned. The most reliable statistics up to the introduction of the Pasteur treatment showed a mortality of at least 16 per cent. from the bite of rabid dogs, so that at least fifty-five lives had been saved from the most agonizing of deaths. Pasteur concluded his paper on the subject before the Academy of Medicine by the suggestion that a vaccine establishment should be set up for carrying on this important work, and others of a cognate character, on a large scale. The project was warmly applauded, money flowed in from all sides, and the magnificent Institution called after Pasteur was built, endowed, and equipped with every requirement needed, not only for the work of hydrophobia, but for researches on every variety of infectious disease, and indeed on microbic life in general. Here his disciples and fellow-workers, Roux, Duclaux, Grancher, Chamberland, Metchnikoff, and many others, found suitable accommodation for the investigations that have thrown so much light on the causation and prevention of disease. The Institut Pasteur was formally opened on 14th November 1888, the occasion being made a great public function, at which our hero may be said to have attained the pinnacle of glory.

His life-work was now accomplished. He entered the Institute that bears his name, already ill and weary. During the years that followed he supervised and directed the labours of his colleagues, but did little more original work himself.

The beginning of the end was in November 1894, when he was seized with a uræmic attack. In broken health he lingered on, tenderly watched over by his wife, children, and grandchildren, till the following September, and then he died. One of his hands rested in that of Madame Pasteur, whilst the other held a crucifix.

The Government decreed him a public funeral at Notre Dame, where his body was temporarily laid to rest. All that is mortal of him now reposes in a beautiful mausoleum, erected at the Institute by his family. The marble arches on either side of the sarcophagus bear inscriptions recording his chief discoveries, whilst beyond it is an apsidal chapel containing a white marble altar. Above the staircase leading to the chapel are inscribed the following words, taken from the oration he delivered at his reception into the Academy of Science:—

" Heureux celui qui porte en soi un dieu, un idéal de beauté, et qui lui obéit—idéal de l'art, idéal de la science, idéal de la patrie, idéal des vertus de l'Evangile."

(Happy he who bears within himself a deity, an ideal of beauty, and who obeys its dictates—an ideal of art, an ideal of science, an ideal of patriotism, an ideal of the virtues of the Gospel.)

Pasteur lived and died a devout Catholic, and in erecting this beautiful monument to his memory, his family took care that it should give expression to the religious side of his nature. The mosaics with which the tomb is decorated comprise angelic figures of Faith, Hope, Charity, and Science. Above the altar we see the descending figure of a dove representing the Holy Spirit, and on either side the Greek letters A and Ω.

As a man of science, Pasteur claimed absolute liberty of research ; but, unlike so many others who get carried away by their speculations, he clung to the rock of objectivity and never attempted to penetrate into primary causes. Whilst, on occasion, making no secret of his repugnance for insolent unbelief and barren irony, he seldom gave expression in public to the ideals which inspired his inner life. The following is one of the few passages in which he alluded to those cherished beliefs and hopes. It is taken from a speech on Spontaneous Generation, delivered in 1874 at the Academy of Science:

"I must not be understood to imply that in my beliefs and in the conduct of my life, I only take account of acquired science; even if I would, I could not do so, for I should then have to strip myself of a part of myself. There are two men in each of us: the scientist—he who starts with a clear field and desires to rise to the knowledge of Nature, through observation, experiment, and reasoning; and the man of sentiment, the man of belief, the man who mourns his dead children and who cannot, alas! prove that he will see them again, but who believes that he will, and lives in that hope: the man who will not die like a microbe, but who feels that the force that is within him cannot die. The two domains are distinct, and woe to him who tries to let them trespass on each other in the so imperfect state of human knowledge." In the words of his son-in-law, M. Vallery-Radot: "With the spiritual sentiment which caused him to claim for the inner moral life the same liberty as for scientific research, he could not understand certain givers of easy explanations, who affirm that matter has organized itself, and who, considering as perfectly simple the spectacle of the universe of which the earth is but an infinitesimal part, are in no wise moved by the Infinite Power who created the worlds. With his whole heart he proclaimed the immortality of the soul."

ALBERT DE LAPPARENT

ALBERT DE LAPPARENT

(1839–1908)

By the Rev. JOHN GERARD, S.J., F.L.S.

Recording the death of M. de Lapparent, our scientific journal *Nature* wrote in terms which may fitly introduce this sketch of his life and work[1]:—

"The loss sustained not only by geology but by science at large by the death of so accomplished a writer cannot at once be fully appreciated. . . . By his death the cause of science has been deprived of one of its most strenuous and successful advocates. . . . He was an eminently religious man, and sacrificed not a little in life for the sake of his convictions. No temptation could induce him to abandon the Institut Catholique, where from its foundation he continued to be one of its pillars."

De Lapparent was the son of staunch Catholic parents, his father being an officer of engineers. He was born at Bruges, 30th December 1839. As he afterwards had occasion to remark, had his birth been two days later, he would have had a twelvemonth more to qualify for official exanimations. In his eighteenth year he gained admission to the École Polytechnique, commencing his course as first on the list of candidates, and afterwards concluding it in the same honourable position. On his twenty-fifth birthday he was nominated *ingénieur de deuxième classe* in the École des Mines, being specially attached at the staff of Elie de Beaumont, whose instruc-

[1] *Nature*, p. 33, 14th May 1908.

tion he particularly esteemed. The master, on his side, had not failed to notice so promising a pupil, who in 1868 was appointed his assistant in the geological survey of France, being thus enabled to serve his apprenticeship under such distinguished auspices in the dolomite district of Southern Tyrol, one specially full of interest and instruction. The memoir prepared on this occasion by de Lapparent was recognized as exhibiting unusual ability, being marked in particular by the precision and lucidity always characteristic of him.

As a result, he was selected by Delesse to assist in preparing the geological résumé annually appearing in the *Annales des Mines*, and summing up the latest advances of the science,—a fortunate appointment, which made him familiar with the results obtained at home and abroad, and furnished him with abundant documentary materials, which he was afterwards to find invaluable. He was likewise able to learn from Delesse himself something of the talent for co-ordinating his various knowledge, for which he was distinguished.

De Lapparent was next employed in the observations required in connection with the famous Channel tunnel, which for some years was under serious consideration. The question was whether there existed from shore to shore a water-tight stratum in which to bore. That such was found on either side of the strait there was no doubt, and the observations of Hawkshaw and Brunel had shown that this extended across the Channel, but the question remained whether it was free from fissures, faults, or dikes, which would suffice to ruin all by admitting the flood. To determine this point, an exhaustive series of soundings and borings were obviously required, and the task was assigned to a commission of experts, Lapparent being its secretary, and the details of its operation being chiefly settled by him. For two seasons, 1875–1876, the work of the commission was diligently prosecuted—nearly 8000 soundings being taken (Hawkshaw

had taken but 800), and the conclusion was reached that there were no flaws in the stratum to mar its suitability for the projected tunnel, the construction of which was therefore quite feasible so far as physical conditions went. For his share in this responsible and toilsome work Lapparent was given the cross of the Legion of Honour.

He was likewise engaged for some years in a geological survey of the district of Bray in Normandy and Picardy, the formation of which, according to de Beaumont, seemed to be expressly designed to allow the nature of the subsoil to be investigated. Here again de Lapparent utilized his opportunities, not only for the accumulation of professional knowledge, but likewise for the preparation of a memoir on the character of the district surveyed, which at once took rank as a model of its kind.

Up to this point he had given proof of qualities which seemed to promise a distinguished career as a practical geologist, who by his own labour was likely to make important additions to our knowledge of his favourite science. He was destined, however, to do his best work and win peculiar distinction in quite another way.

In 1875 he was offered and accepted the post of Professor of Geology and Mineralogy in the Catholic University of Paris, then being organized, and was granted unlimited leave of absence from his duties in connection with the École des Mines. But, in 1880, when anticlericalism became militant, he was curtly informed that he must decide between his official position as a state engineer and that of professor in such a university. As a staunch Catholic and strenuous upholder of educational liberty, he did not hesitate to sacrifice what might have appeared the best prospects of his life by refusing to resign his chair; and although it might have been thought that he thus wrecked his whole career, the step proved to be in reality a most fortunate one, no less for himself personally than for the world at large. He was now at

full liberty to devote himself to a work which he had already projected, which should exhibit the fullest and most authentic results of geological knowledge brought thoroughly up to date, and guaranteed by the best authorities. For such a task he had exceptional qualifications. In the actual works in which he had been engaged he had learned in practice how geology should be studied, and its conclusions be reached. His share in preparing the geological map of France had given him detailed information as to the various formations of his native land, while for fifteen years his co-operation in the *Revue de Géologie,* conducted by Delesse, had supplied him with a wealth of material from all quarters, as valuable as it is hard to obtain. Moreover, he was distinguished even amongst his countrymen for that perspicuity and literary charm which are so marked and admirable a characteristic of French scientific writing.

His first and most important publication was the *Traité de Géologie,* which appeared in successive parts between 1881 and 1883. This had at once a great success, which was due partly to the manifest need of such a work; for hitherto there was no source from which to draw geological information in France but works which were antiquated and usually defective, often translated from other tongues, and inspiring no confidence. Still more effectually was the *Traité* recommended by its own merits, which none could fail to recognize, by its luminous presentation of every department of science, its admirably logical arrangement, and its characteristic elegance and limpidity of style. In the space of 1200 pages the author contrived to distil the substance of countless memoirs in all languages with a fulness and clearness which left nothing to desire, so that each of his chapters became an encyclopædia of the fullest and most recent advances of geological knowledge in every branch, as, for example, with regard to microscopic evidence lately obtained upon the constitution of

eruptive rocks. The first edition of 3000 copies was speedily exhausted; a second of 4400, appearing in 1885, was not only greatly improved and augmented, but had its text carefully revised and edited. As in all his works, the author's great object, as he declared, was to make himself a faithful expositor of the actual state of his science, and he never hesitated to abandon views previously expressed if he thought that more recent observation failed to corroborate them. The fifth edition, in three volumes, with an aggregate of over two thousand pages, which has taken its place as an indispensable book of reference and suggestive guidance to every student of modern geology, contained much additional matter required by the advance of knowledge. For since 1881 the area of the globe's surface to be dealt with had been more than doubled, while travel and personal observation, and information gathered from experts at international congresses, had furnished abundant material. Besides all this, a feature particularly notable was the introduction in the text of numerous maps to illustrate the geographical features of different regions in successive geological periods, as to which we are told [1] that no one can peruse these restorations without a sense of the enormous amount of geological literature which had been laid under contribution, and that, although they could only be tentative, the data being often meagre and not always trustworthy, they are replete with interest and suggestion.

Besides the great work of which we have spoken, de Lapparent produced a Compendium of Geology, designed principally for beginners, but proving of great utility to teachers as well as pupils.

Having found it necessary as professor to turn his attention to mineralogy, a subject in which, as he himself declared, he found unexpected interest, and even fascination, he produced a treatise on this subject ex-

[1] *Nature*, l.c.

hibiting qualities like those of his geology, and attaining a similar popularity, especially abroad: it reached a fourth edition in 1908, the year of the author's death. He likewise did much to promote the study of physical geography, so closely connected with geology; and, amongst other contributions to this subject, produced his *Géologie en Chemin-de-fer,* giving an account of the various formations through which the railway had to pass within the Paris basin, and the features presented by the different soils so traversed. All these works, distinguished alike by lucidity of arrangement and elegance of expression, have proved, we are assured,[1] of the greatest service in furthering the progress of science in its several branches and the general advance of education.

In 1907 he received the high distinction of being appointed Permanent Secretary to the Académie des Sciences in succession to M. Berthelot.

Being thus in continual touch with the views of geologists in all parts of the world, it was inevitable that de Lapparent should be confronted by the questions which the study of this science necessarily raises, and which, as we are often told, make it more than any other impossible to reconcile with Christian belief. From the history of his life it is evident that his own faith was nowise impaired by such studies; and, moreover, he constantly employed both tongue and pen in defence of what he held to be the cause of truth, both in numerous magazine articles and in addresses to Catholic scientific congresses in various regions. Some of his contributions have been issued separately, and furnish material well worthy the attention of all who have occasion to discuss such subjects.[2] Profoundly assured that all truth is from God, whether religious or scientific, and that there can therefore be no real contradiction between its lessons,

[1] *Nature*, l.c.

[2] See *Science et Apologétique*, 4th ed., 1910, and *La Philosophie minérale*, 1910.

he was quite undisturbed by the difficulties which some find so formidable, and he was always confident that with fuller knowledge they would be dissipated; nor would he ever consent to attempt by forced interpretations to seek a reconciliation between the Scripture narrative of Creation and the geological records. Neither had he any sympathy with those who, regarding science with suspicion and hostility, sought to belittle its conclusions by pointing to cases in which the haste of some extreme partisans to find matter for theories adverse to Christianity had betrayed them into manifest error. Thus, after discussing the case of what are described as eoliths, stones declared by some to be articles of human manufacture, which would extend the period of man's presence on earth not merely by hundreds but by thousands of centuries, after contending at length that their artificial character is inadmissible, and is supported by arguments in which fancy plays a predominant part, when the discussion had wound up with the account of what, in his own opinion and that of other experts, proves to demonstration that objects precisely similar to the supposed eoliths can be formed in a purely mechanical manner, without the introduction of any intentional element, Lapparent thus goes on[1]:—

"To find fault with the extravagances of which some men of science may be guilty is not to attack science herself, and the detection of such mistakes does not by any means justify us in assuming an attitude of suspicion in regard of an edifice whereof some portions may be defective without at all impairing the efficiency of the rest. When we narrated the diverting experiences of the eoliths, there were not wanting some who, taking note of this exposure, jumped hastily to the conclusion that the history of all chipped flints without exception was to be treated as fabulous. Nothing is further from our thoughts; we feel bound to repeat once again that

[1] *Philosophie minérale*, p. 311.

in writing the foregoing pages our object was not in any way to cast discredit on prehistoric archæology. On the contrary, we gladly acknowledge that it has in truth done wonders, and enriched our minds with a long series of particulars as full of interest as they were before unsuspected. Our only object has been a desire to put right-minded men on their guard against the proceedings of a school which, notoriously influenced by anti-religious zeal, has manifested far too great readiness in admitting what has never been actually proved, a school all the less entitled to adopt such a course that its representatives are for ever declaiming of scientific methods and 'positive facts.'"

From this attitude de Lapparent never departed, ever maintaining that science, if genuinely scientific, is entitled to honour and respect, and that its results must be loyally and ungrudgingly accepted. He thus treats the great question which in our day more than in any other has exercised the minds of men [1] :—

"The Christian apologist should be well-informed, not that he is bound to have complete acquaintance with all the various sciences, which would entail superhuman labour, but it is imperative that he should keep himself abreast of the results achieved in each department, and not be guilty of the error of employing inadequate weapons drawn from antiquated sources, or from information supplied by the hasty perusal of popular works possessing no authority.

"In this connection we will specify the results of a science whereof a century ago there was no inkling, but which for the future must remain as established beyond all contradiction, and has now acquired enormous developments, namely, geology. Unquestionably there still remains much about it which we do not know, and in each of its chapters differences arise amongst specialists which occasionally lead to lively controversy. Never-

[1] *Science et Apologétique*, c. vii.

theless, the main lines of the doctrinal system become better established every day, and in regard of some questions that unanimity has been attained which is the guarantee of empirical certitude.

"Amongst these is the great antiquity of the globe. It will doubtless be long before we can fully estimate the period required, but that, for the whole process of formation, it must be reckoned in millions of years there can be no question.

"Neither can we fail to recognize on the surface of the land and in the seas the constantly recurring succession of organisms which differ more widely from those now existing in proportion as their antiquity is greater. Nor will any competent geologist of the present day suppose that these various generations of diverse animals and plants have disappeared under the action of violent catastrophes.

"Here the geologist appears to give valuable evidence on the subject of evolution, which is so much discussed and yet remains so obscure. Since the different forms occur in regular succession, and intermediate types are never wanting, while their variations are ever in accord with the difference of their ages, it is very difficult to avoid the idea of an evolution, regulated like all else here below, and under the control of a potent cause beyond Nature, according to a determined design. Of course, geology can never supply a direct proof of this, since it is acquainted only with fossil remains, and has not the opportunity of exhibiting Nature at work. Still, the impression resulting from consideration of the realm of palæontology seems impossible to reconcile with any other system. Therefore, without at all pretending that the question is finally determined, and fully acknowledging that as yet the machinery of the transformation is wholly beyond us, we are of opinion that the apologist would be ill-advised who should assume, in regard to the principle of evolution, the combative and irreconcilable

attitude which it has frequently been thought necessary to adopt."

A further observation may be commended to some controversialists :—

" May we likewise be allowed to indicate another duty to which the apologist is specially bound if he desires his work to bear fruit, that, namely, of imbuing himself with that tranquillity which should always dominate scientific work, and with which it is not well to dispense, even under plea of giving free scope to legitimate indignation. Sound reasons do not require to be expressed in violent terms, and conclusive arguments gain nothing by an exhibition of temper or petulance."

While he did not shrink from criticizing the methods sometimes adopted by champions of orthodoxy, it need not be said that he never hesitated to express his opinion that their opponents frequently represented as scientifically established that which was in truth recommended by its accordance with their own doctrines rather than by more solid reasons, and which lost instead of gaining ground as knowledge advanced. So, as to the vexed question of the antiquity of our race, in which he took particular interest, treating it on several occasions from various points of view, he thus expressed himself [1] :—

" What we have endeavoured to make clear is that positive science, the better she is informed, and the more independent she is of prepossessions, that science which is in no hurry to come to conclusions, and demands exclusive proofs, tends to discredit rather than to corroborate the larger estimates of time which many delighted in representing as final. Whether the question be of eoliths, of fossil men, or of the date of the palæolithic centres, we find that the most trustworthy observations agree in assigning a later, not an earlier, date to the first authentic indication of human activity. The moral to be gathered is a decisive enforcement of that caution

[1] *Philosophie minérale*, p. 313.

which is too easily forgotten, but of which true men of science should never be unmindful."

In many ways de Lapparent showed himself a remarkable man, who gained general regard. As testified by the obituary notice already cited, there was something in him eminently attractive; his gentle and kindly manner drew to him men of all nationalities, and his charm as a speaker led to his being continually in request to deliver public addresses, in which the well-modulated voice, the felicitous choice of words, and the flashes of humour, made his speeches delightful to listen to, while, under playfulness of speech, public and private alike, he would from time to time reveal the depths of his serious nature. He spoke and wrote German with facility, and at Berlin in 1899 and Munich in 1900 addressed scientific congresses in that language.

As for the Institut Catholique, for whose sake, as we have seen, he was ready to make such serious sacrifices, how seriously he took his obligation in its regard was evidenced when he showed himself prepared to traverse the whole extent of Paris in order to give instruction to a single pupil.

When, in 1894, to commemorate the centenary of the Polytechnic School, a series of celebrations were arranged, from which everything of a religious character was entirely eliminated, some of its former members determined to do what they could to supply the deficiency; and though he disclaimed the credit of originating such a project, it was undoubtedly to de Lapparent's cooperation and energy that its striking success was due. Not only did a numerous company assemble to cherish the memory of their old comrades, but the Mass then inaugurated has become an annual institution, which brings together considerable numbers, and has elicited demonstrations of sympathy even from non-Catholics, who still believe in the immortality of man.

While he was unmatched in the work of popularizing

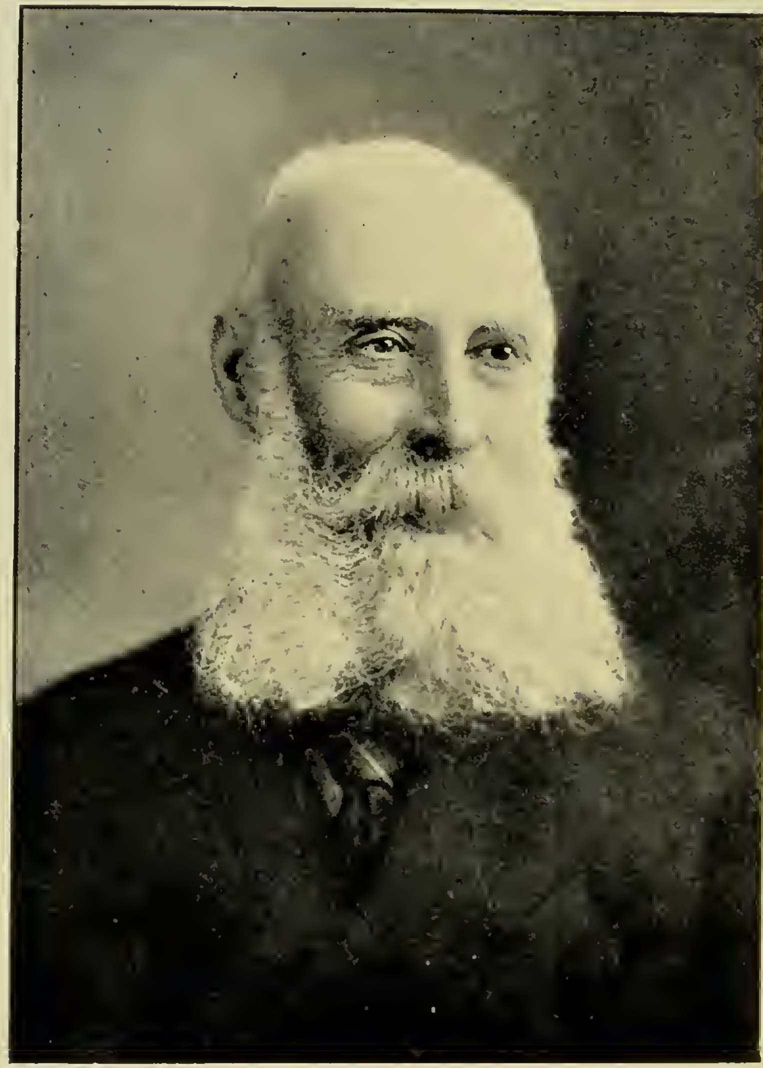
science in the interest of religion, there was nothing in de Lapparent's pages of the arid and prosaic character which seems so often to be induced by studies like his. He knew how to make science attractive and intelligible to educated minds without derogating in any degree from accuracy ; and in his apologetic work it may be said that every page is illumined by evidence of the power and wisdom to which the world owes its being.

But, except in rare and peculiar cases, he did not, when dealing with questions properly scientific, make explicit mention of the supreme intelligence to whose control Nature bears witness. He left facts to speak for themselves, and was satisfied with making plain the great notes of order which stamps Nature as, beyond question, the handiwork of God.

When, however, he had the opportunity of rendering a service to the cause of religion and of science, he never failed to exert his voice and pen on their behalf, employing both again and again, in international scientific congresses, and others of many kinds, in homely popular lectures for the benefit of unlearned folk, in articles contributed to reviews and magazines, and in conferences at his own Institut Catholique.[1] So hard did he work, up to the very end, as to make him humorously wonder how it was that eyes were still left him and he had escaped writers' cramp.

To him might assuredly be applied what he said of L. de Bussy in 1904, that the lives of some men furnish sermons more eloquent than those of the best preachers.

[1] J. Muthuon in the *Études*, 20th July 1908.



THOMAS DWIGHT

THOMAS DWIGHT

(1843-1911)

BY

SIR BERTRAM WINDLE, M.D., Sc.D., F.R.S., etc.

THE visitor to Stratford-on-Avon, when passing down High Street, has his attention diverted from the otherwise all-absorbing presence of Shakespeare by a fine specimen of a sixteenth-century house with elaborately carved barge-boards known as "The Harvard House." This house was built by Alderman Thomas Rogers in the year 1596. His daughter Catherine married John Harvard. Their son, also a John, graduated at Emmanuel College, Cambridge, in 1635, took Anglican Orders, went to New England and died, whilst still quite a young man, in 1638, bequeathing to a College in New England, which it was then proposed to erect, his library consisting of over three hundred volumes and a sum of £779. The College, when founded, was given his name and is now the well-known Harvard College, situated at Cambridge, near Boston, the oldest and perhaps the most celebrated seat of learning in the United States.

The Chair of Anatomy in the School of Medicine connected with this College was founded in 1782, and its first occupant was one John Warren, whose brother, General Warren, was killed at the battle of Bunkers Hill. John Warren was succeeded in the Professorship of Anatomy by his son John Collins Warren, who in turn gave place to Oliver Wendell Holmes, a name more distinguished in literature than in science. Holmes in his turn was succeeded by Dr. Thomas Dwight, the subject of this paper, who was a grandson of John Collins Warren, and consequently the great-grandson of John Warren, the founder of the Medical School at Harvard.

Thomas Dwight was born in Boston on October 13, 1843. His father, also a Thomas Dwight, was the representative of the seventh generation of that family in New England. Thomas Dwight, senior, graduated in Harvard in 1827 and died in 1876, having been received into the Catholic Church during his last illness. His wife was the daughter of Professor John Collins Warren, the founder, as will shortly be shown, of the Warren Museum, and a man of strong religious feeling. This was shown by the fact that when in 1820 the religious body to which he belonged began to turn itself in the direction of Unitarianism, Dr. Warren refused to follow, and with a few friends established St. Paul's Church. "It took courage," says the account which I am quoting from Dr. Harrington's address to the members of the guild of St. Luke's, Boston, "to disagree openly with what practically amounted to the State religion at that period." The same writer points out that in 1855 "strong courage of religious feeling, to say the least, was required for a non-Catholic to embrace the Catholic faith in Boston." Yet it was at this time that Dr. Warren's two daughters, Mrs. Charles Lyman and Mrs. Thomas

Dwight, became Catholics. At the same time, Mrs. Dwight's son, Thomas, the subject of this paper, was also received into the Church, being then aged thirteen. In his *Thoughts of a Catholic Anatomist*, Professor Dwight refers no doubt to some extent to this event when he says "It is now the fashion, in a certain set, to declare that religion is an emotion. Nothing could be more fantastically absurd nor more untrue. . . . When a man considers deliberately whether he will accept the doctrines of the Catholic Church, and having prayed for guidance in making his decision and for strength to stick to it when made, he may or may not experience an emotion (probably he will experience a great emotion), but his action is not the result of emotion; on the contrary, it is the cause of the emotion. Suppose he remains true to his religion through great trials, from which he could free himself by being false to his convictions, have we any right to say that this is emotion? There are plenty of instances of men and women persevering faithfully for long years in austere orders, in which the life is of the hardest, without the support, the sensible fervour which is granted to some. They went on when all was hard, pleasureless, nay, repulsive. Was Father Damien's long work among the lepers the result of emotion? That religion may awaken emotion is most true, just as exercise may awaken appetite, but the emotion is no more the religion than the appetite is the exercise. . . . In point of fact, it is hard to see how any thinking person can seriously support this view."

Thomas Dwight, junior, was educated at a private school, and entered Harvard in 1866. Possibly his medical ancestry on his mother's side turned the young man's attention to that profession. Be that as it may, Dwight graduated as a medical man and shortly turned his attention to the scientific side of his profession, his first essay in that direction having been a study of the

subject of intracranial circulation, then a matter of great medical controversy, for which he was awarded first prize by the Boylston Medical Society in 1867. In order to improve his knowledge the young doctor spent two years in study at Vienna and Berlin, and subsequently went to Munich, where he worked under Rüdinger, the originator of the frozen-section method which threw such a flood of light upon topographical anatomy. On returning to America, Dwight was appointed Professor of Anatomy at Bowdoin College, where he introduced the frozen-section method for the first time in America. During his occupancy of this position he was called upon to give evidence as to identification of bones in a celebrated murder trial. Dr. Dwight's evidence was regarded as of the highest importance, and the study of the subject which he then made led to the production of his work on *The Identification of the Human Skeleton.*

In 1872 Dr. Dwight was appointed instructor in Comparative Anatomy at Harvard, and, after holding several other comparatively minor posts, was, in 1883, elected Parkman Professor of Anatomy in the room of Oliver Wendell Holmes, who had occupied that position for thirty-five years. Holmes was essentially a popular lecturer. He is known to the world as the writer of delightful literature, and most prominently as the "Autocrat of the Breakfast Table." But so far as I am aware, he is quite unknown to the scientific world by any contributions to the literature of anatomy. Dwight, on the contrary, set himself to the task of treating anatomy as a science, especially a statistical science, and, above all things, as the basis and foundation of medical teaching and practice. "For practically the first time in the history of the school," says Dr. Harrington, "original work was undertaken by students and teachers. Anatomy became a science; dissection an art. Most minute statistical observation of the variations of structure,

number, and arrangement of bones, organs, and tissue were collected and collaborated. The course of anatomy was systematized so that the first-year students were taught descriptive anatomy, while lectures and exercises on topographical anatomy were confined to the more advanced classes." As will be seen later, Dr. Dwight held the Chair of Anatomy until the time of his death in 1911.

It is more than a little difficult to give to the non-scientific reader any clear idea of the services to science of a man occupying a position such as that held by Professor Dwight. It has aptly been observed by someone that a man may hold the highest possible position amongst his scientific compeers without his name being known in any way to the general public. This is perhaps essentially the case with an anatomist who, at the present day, is not in the way of making those startling discoveries which render such names as those of Lister, Pasteur, or Röntgen household words. Nevertheless, such a man may contribute, as Dwight did, in various ways and in large measure to the advancement of knowledge. In this connection allusion may first be made to his relation to the Warren Museum. This museum had been started in 1799 by his grandfather, Dr. John Collins Warren. It must have been therefore in every way a congenial task to Dwight to devote himself to its arrangement and classification. This task to which he set himself on his appointment to the Chair of Anatomy was continued up to the time of his death; his contributions to the collection were constant and numerous, and this great museum will always remain a monument of his patient and careful organizing capacity. Before passing away from this collection it should be stated that it contains an extraordinary variety of normal and morbid specimens, amongst which is the famous "crowbar skull," a puzzle psychologically no less than surgically. It came from a

man through whose head a tamping iron was completely driven by the premature explosion of a blast. He lived for thirteen years afterwards with the loss of one eye, but with unimpaired intellect. Much of the anterior part of the left frontal lobe of the brain must have been destroyed, yet his speech and memory for words were not affected.

Dwight's public work mainly dealt with the subject of variation and particularly with variation as related to the skeleton, and more especially to the spine, hands and feet. For years he was engaged in making a very valuable collection of human spines showing practically all possible numerical variations of the ribs and of the vertebræ in different regions, and of fusions between different parts. The results of these researches appeared in a memoir of the Boston Society of Natural History and in contributions to other anatomical journals. He also studied and described the abnormalities at the top of the spine which might cause malpositions of the head and face. After completing his studies on variations of the spine, he devoted himself to the same subject in the hand and foot, and succeeded in obtaining a remarkable series of specimens showing the chief variations in the carpus and tarsus, and including several unique cases of variations in these regions. He was the first to find and describe the subcapitatum as a separate and distinct element in both hands. This was especially satisfactory to him, as Pfitzner had described the possibility of the separate existence of this element, but had never seen a case of it. In the foot he found two cases of an absolutely new element, the intercuneiform bone, which had never before been observed, and also two instances of the secondary cuboid bone. The first of these occurred in one foot, and only one previous case had been seen by Schwalbe. The other occurred in both feet and was a unique case. In 1907 Dr. Dwight

published an atlas on the variations of the bones of the hand and foot, based on the specimens in his collection and on X-rays. This careful and detailed work on variations naturally gave great weight to his opinion on the present day controversy as to the relative values of small variations and greater changes, or, as they are now called, " mutations," in the process of Organic Evolution. He gave one of the six addresses on this subject before the American Society of Naturalists at Philadelphia in 1905. In this address he commences by pointing out the essential difference between Darwin's idea and the idea of de Vries. " A radical difference," he says, " between the two theories is this : Darwinism pure and simple is essentially fortuitous ; it aims in no particular direction, there is no goal ; while mutation by producing suddenly a new species, or at least a subspecies, implies the existence of a type and of a law which under certain conditions becomes operative." Further he proceeds, " The theory of change by minute variations receives no support from anatomical observations. Precisely what many thought an illustration of Darwinism is its refutation. Huxley foresaw this when he doubted whether variations might not prove a two-edged sword. The fundamental error into which supporters of evolution by selection are logically driven is the unwarranted assumption that similarity of structure can be explained only by descent. Though not formally stated, this is tacitly accepted almost as an axiom." And he concludes, " It is to my mind impossible to find any support for a theory of evolution by minute changes from the study of anatomical variations. I should not venture to say, on the other hand, that they give any direct support to the theory of mutation ; but at least they are not in disaccord with it."

It would be tedious and indeed useless in a popular account such as this to deal particularly with Professor

Dwight's scientific communications. Suffice it to say that to the account of his life given in the *Anatomical Record* is added a bibliography of his writings, the items in which amount to seventy-six in number. It may be added as proof of the value of his work that Dwight's colleagues in science gave to him the highest position in their power when they made him President of the American Anatomical Association.

In addition to these mostly purely professional publications mention must be made of Dr. Dwight's writings of a Catholic or of what is called an "apologetic" character, amongst which by far the most important is his *Thoughts of a Catholic Anatomist*, which might almost be described as a posthumous work, since it appeared when its author was on his death-bed. To this work further attention will be paid in a later part of this essay, but before dealing with it it will be necessary to say something about Dr. Dwight's position as a Catholic; and in dealing with this aspect of his life, I must of necessity largely draw upon the information supplied to me by the series of addresses delivered before the members of the Guild of St. Luke at Boston, a guild of which, by the appointment of Cardinal O'Connell, the subject of this biography had been constituted the first President.

Dr. Dwight, as I have already mentioned, was received into the Catholic Church in his thirteenth year, and of that Church he remained a fervent and attached member during his life. Those who read his works will gather that he was not only a devout Catholic, but will see that he was also an instructed Catholic; he was a deep student of the works of St. Thomas Aquinas, and fully appreciated the overwhelming claims on the attention of intellectual men and women which our Church, in addition to a thousand other reasons for our affection, possesses in such a remarkable manner.

That this attachment to his Church was not a secret possession, but that his faith was as a city set upon a hill to be seen of all men, is abundantly clear and is indeed admitted by the writer of his biography in the *Anatomical Record,* of course a purely scientific periodical, when he says, " His deep religious feeling and his devotion and loyalty to his faith were his strongest characteristics and influenced to a great degree his opinions and his scientific point of view."

" I would rather be thought a bigot than to be too lax," was a favourite and characteristic remark of his, and it is recorded of him that when a newspaper, hoping no doubt to say something which would please the Professor, described him as a " Liberal Catholic," he wrote to its editor to say that he knew of nothing which he had ever said or done which would render such an epithet justifiable.

With these introductory remarks I may now pass to the series of addresses of which I have already spoken. They are four in number, and were delivered by medical men of Boston who considered various aspects of their late President's character, especially, of course, in its relation to the Catholic faith. No sketch of Dr. Dwight, says Dr. Harrington, can touch the secret of his strength nor understand his actions without a consideration of his intense Catholic faith. In fact, his ardent Catholic faith was his life. It gave outward expression to the intense spiritual nature which permeated his whole inner self. That he was often misunderstood and frequently misjudged by those who could not appreciate such a character, is not surprising. That a religious faith such as he possessed and manifested should invite bigotry, opposition, and at times persecution, few realised better than he. If any of these poisonous arrows ever caused him the least personal anguish or pain, no one ever knew it. The beauty of it all is that he was so unconscious of the moral

courage he so constantly practised. Militant Catholicism was as real to him as militant patriotism was to his Warren ancestors. There could be no compromise on either. This spirit gave to everything he did—religious, professional, and lay—a life and colouring far outside the ordinary. Hating sham, despising hypocrisy, and shunning notoriety, he never tolerated any of these from those associated with him, nor from those claiming to speak with authority. Possessing the honest scepticism of the real scientist, he was at the same time a most rigid disciplinarian, once authority was established. In the many perplexities into which this principle often carried him, he always found strength in the wise, conservative authority of the Church as expressed by the Council of the Vatican: "Nulla unquam inter fidem et rationem vera dissentia esse potest" (Never can there be a real conflict between faith and reason). It would have been quite unnatural if a nature so deeply religious, and one so ardent and militant in its Catholicism, remained silent when the tenets of that religion were attacked or neglected. Such occasions, happily less frequent to-day, never found Dr. Dwight timid, unwilling, or unjust. He was ready to grant to others the right he demanded for those whose cause he championed. He would accept no less than justice and the law guaranteed. As the sole representative often of the Catholic faith on State boards, he fought valiantly and successfully for the parental and religious rights of those wards committed to the State's care; as a Catholic citizen, he protested against State interference in the choice of education by parents for their children; as trustee of the Boston City Library, he fought the proscription of Catholic authors, as well as the introduction there of blasphemous, debasing, anti-Catholic literature. Numerous instances might be recalled where Dr. Dwight publicly righted popular prejudice and misconception on the position of the

Catholic Church in science, in works of charity, and in federation. One occasion deserves special mention. It was the Faneuil Hall meeting in 1907, protesting against the injustice of the French Government towards the Catholic Church. His oration on that occasion, we are told by those who heard it, recalled those stirring, vigorous, patriotic addresses of his colonial ancestor, John Warren.

A further example of this desire to put matters right when the Catholic Church had been attacked may be found in the incident which led to the publication of his paper *Commonplaces of History*, which was called into existence as a reply to an essay which had been contributed by a gentleman to a social club in Boston on the subject of the Spanish discovery of America. This paper incidentally pointed out, according to Dr. Dwight's summary, "that there was no redeeming feature in Spanish rule in America; that the Catholic Church was responsible for the evils; that the treasures of the New World, by enriching Spain, enabled it to carry on for so long the bloody wars of the Netherlands; that they thus supported throughout Europe the Catholic cause against the Protestant, the former of which stood for oppression and abomination of all kinds, while the latter represented civil and religious liberty. Finally, that we in America are indebted for those blessings to our Puritan forefathers."

These accusations Dr. Dwight set himself to confute, and it is pleasant to be able to relate that the person who suggested that he should prepare his essay in reply was the author of the original attack. Dr. Dwight narrows down the controversy to the following points. "Is the Catholic Church to be held accountable for the misdeeds of those who were her children but in name, and have we to thank the Reformation for civilization, for freedom from tyranny, and for liberty of conscience?" It is

not possible to follow here his enlargement of the thesis laid down, but it is of an exhaustive and convincing character and an excellent example of the late Professor's method of handling facts and of standing forth as the Champion of the Church when she was attacked.

Dr. Dwight was an active supporter of the Society of St. Vincent de Paul, and, oddly enough, it was through this that I first became acquainted with the fact that he was a fellow-Catholic. Dr. Dwight's chief work, as I have already shown, was largely in connection with the subject of variation, and that was a subject to which I also at one time directed a good deal of attention, and in connection with which I published a number of papers and abstracts. These communications I was in the habit of sending to Dr. Dwight, as a fellow-worker, and from him at various times I received in exchange copies of his writings. Amongst these, on one occasion, arrived the Annual Report of the Boston S. V. P., with the "Compliments of Thomas Dwight." To my great surprise I then discovered that my friendly correspondent of a number of years was a brother in the faith, and our communications for the future became less entirely of a scientific character than they had been heretofore. Amongst other papers which I received from him was a card printed and distributed by himself in honour of the silver jubilee of the Archbishop of Boston, on one side of which was St. Thomas Aquinas's prayer, "Creator ineffabilis," which might well be the prayer of every man of science.

But to turn to Dwight's work as a Brother of St. Vincent de Paul; and here again I must derive my information from the addresses of those who worked with him and knew him personally as it was never my good fortune to do.

Dr. Dwight was for many years a Brother of the

Society of St. Vincent de Paul, and for a considerable time occupied the post of President of the Central and Particular Councils, a position which he accepted, though with many regrets that it should have deprived him of that personal satisfaction he had experienced when a conference visitor to the poor. He was in the habit of frequently referring to the gratification which these visits gave to him, and he used especially to indicate the pleasure which he derived from the visit on Christmas Eve, "when the members assembled at Mr. Williams's store and each man received his basket for delivery to the widowed, the orphaned, or the sick."

He was also constant in his endeavours to obtain new Brothers, and one of his recruits, who was also one of his students, in delivering one of the addresses to which I have so frequently alluded, gave an account of the way in which he came to be enlisted in the Brotherhood. When a medical student, he tells us, Dr. Dwight suggested to him that he should join the Society. "Several interviews with him," continues Dr. Leen, the Brother in question, "showed me that the sanctification of one's own soul was the only reward of membership. He pointed out that though I might be a busy medical student, the work under ordinary circumstances did not require much time, and that, except in the matter of meetings, one can generally choose his own hours. He argued that students and professional men have the time, for most of us find time to do what we want to do. It was particularly regretted that of the many young Catholics of superior education so very few seemed called upon to join the Society, in contradistinction to the non-Catholic benevolent societies which contained among their members so many of the best minds of the community. There was constantly in his mind the loss those not members suffered, for he would say one can bring nothing to the Society of St. Vincent de Paul that bears any comparison to what he

receives from it; and there was nothing which gave him greater pleasure than to welcome young men to its ranks where they could devote the fruits of their education and talents to the honour of God by serving his poor." But the late Professor's opinion as to the Society may be better learnt, perhaps, from his own words than from those even of his warmest admirers. At a general meeting of the Society he laid before the Brothers his own idea of the Society in words which may well bear quotation here. "If our Society should cease to exist," he said, "there would at first be a great void; new machinery would have to be provided to maintain the poor, and very large sums of money would be needed; but I have but little doubt that after a certain time the physical needs of the poor would be fairly well provided for by others, so that the superficial observer without the insight of faith would see no great loss. He would not know of the family here and there which has been brought to a good life, of the dying sinner who has received the sacraments, of the children whose faith had been saved, of the prisoners who had been visited, of the self-denial of the member who had done his work at great inconvenience, of the good examples given of the graces and indulgences gained. This is the supernatural side of our work which makes it truly worth the doing."

One other piece of religious work in which Dr. Dwight delighted must not pass unnoticed. This was the Society of the Nocturnal Adoration of the Blessed Sacrament, which was established by himself at the Boston Cathedral in 1882. He and a small band of associates met on the eve of the first Friday of each month and on the opening of a Forty Hours' Adoration. Each member in turn kept his vigil before the Blessed Sacrament during the exposition, and all received Communion at the Mass. Cots were provided in the vestry for those members

awaiting their hours during the night of sacred sentinel duty. This practice he continued until his failing health made such a service unwise.

This failing health was first made known to me in a letter which gave me a great shock, for at the time I received it I had no idea that the Professor was not in full vigour. I had written to him on behalf of a young friend and former pupil, then taking out a post-graduate course in Harvard University, and his letter in reply began with some remarks on this matter. It is dated April 20, 1910, and the portion of it which I am about to transcribe relates to his last illness. He says, "I enjoyed very much meeting Professor Macalister[1] last June. We had a grand time with my variations. It is not often that I can show them to one who knows. Cunningham[2] was then at the point of death, and Macalister told me that he had cancer. It seemed to me that the thought (in substance) passed through my mind, 'hodie tibi, cras mihi.' Then I had only suspicions about myself; now I know. It is, however, a slow-growing affair, so they tell me. I have done a good deal of work this season and hope to be able to stand it one season more. The Harvard authorities have behaved far more than handsomely. Perhaps you will pray for me sometimes."

The courage and resignation to God's will shown in this letter were exemplified during the remainder of his life. The *Anatomical Record*, in the obituary from which I have already quoted, says, "The last two years of his life were passed under the handicap of an incurable disease, in spite of which he gave two full courses of lectures and added several monographs to his work on the skeleton. . . . He always looked forward to meeting

[1] Alexander Macalister, F.R.S., then and now Professor of Anatomy in the University of Cambridge.

[2] D. J. Cunningham, F.R.S., then Professor of Anatomy in the University of Edinburgh.

his classes at the first exercise in their medical career, and, although in the summer he knew that he was failing, still hoped to meet this year's class at least once in the fall. This opportunity was denied him, and his death occurred three weeks before the opening of the term. To all who had the privilege of being associated with Professor Dwight in the Department of Anatomy, his courage in persisting in his work while suffering great discomfort, and at times much pain, was a most inspiring example. His ability and determination to show no sign of weakness enabled him to make the best possible use of the short time that was left to him. It gave him the satisfaction of continuing his work as a lecturer as well as his contributions to anatomical science to the very last, and was a worthy climax to his long and laborious career."

From another point of view, that of his religious associates, this picture of his last days drawn by one who had been his anatomical colleague may be completed. The obituary notice in *America* says: "There could be no finer summing up of such a life as Dr. Dwight's than the record of the two last years. Just two years ago he knew that he was attacked by a fatal disease. He accepted it, not only with calmness and courage, but with abundant cheerfulness, and wished that no secret should be made about it. He kept on with all the work for which his strength sufficed, and he often said that his health was so much more than it was reasonable to expect, that it must be supernatural—a direct answer to prayers. He was an object lesson to all, and called forth admiration from Protestants as well as Catholics, some of the former saying that if the Catholic religion could make a man and all his family receive affliction in such a spirit, it was a faith that *all* must reverence. Though failing all summer, at times he could rally enough to say, 'I think there is a fighting chance that I may give my lec-

tures again next winter.' Those he gave last winter were up to his highest average. But those near him knew that there was no 'fighting chance' left for him in this world. The whole community at Nahant was 'as one family,' it was said, as this truly consecrated life was ebbing away; and when the last breath was drawn, human ears could almost hear the 'Well done, good and faithful servant' which must have greeted Thomas Dwight on the other shore."

It was during this period of pain and work that Dr. Dwight prepared and published his *Thoughts of a Catholic Anatomist*, which, coming as it does as a kind of legacy, may well be considered in the concluding lines of this brief biography. In its preface he calls attention to the fact that it is often said by those outside the Church that they cannot see how a Catholic can be a man of science and, conversely, how a man of science can be a Catholic. It is to establish the contrary of both these propositions that this series of lives of Catholic men of science has been taken in hand, and no better example of the kind of man who not only could be but was a devoted son of the Church, almost one might say a "religious" living in the world, and at the same time an exact, enthusiastic man of science, could possibly be desired. And his book is the reflex of his double capacity. Returning to the preface, he admits that he fears that there may be many poorly instructed Catholics who hold the same false idea as to the incompatibility of Catholicism and science. And then, alluding to the two classes of persons of whom he has been speaking, he continues: "It may be that it is my duty, on account of the position I have the honour to hold, to give to both these classes such poor help as I can." In the earlier pages of his book he laments that Catholics influence public opinion so little as they do, and that they are not sufficiently to the fore in the matter of proclaiming the message

which the Church has for the world, scientific and non-scientific.

Further, he contends, and rightly contends, that we should not content ourselves with merely opposing false opinions, but should constantly endeavour to bring forward the better principles of which we ourselves are in possession. His views on this point may be briefly summarized in a quotation from his introductory chapter. " I incline to sympathize with the sneer of a reviewer who, in the discussion of a book maintaining that there is nothing in religion contrary to science (or indeed in science contrary to religion), exclaims, 'Nothing contrary!' as one would say, 'Is that all? Have you nothing better than that?' It seems to me that many of the apologists for Christianity have made the mistake of fighting too much on the defensive. They have held their position, they have shown the weakness of their opponents; but, if I mistake not, they for the most part have stopped there, without going on to show that, as far as science has anything to say in the matter, its evidence is in support of religion, and that as a whole the Catholic view of nature and man is grander, more logical, and more satisfying than that of the monist."

These few paragraphs are not intended either to contain a synopsis or to act as a review of the *Thoughts of a Catholic Anatomist*. It is a book that should be on the shelves of every thoughtful man, and those troubled with scientific difficulties will find within its pages much to help them in their hour of trouble. Dr. Dwight was a man of firm faith; trust and confidence in God and the Church which He has founded for our help and consolation were the sure ground on which he rested, and he was not the very least bit afraid to face any scientific theory, however daring, and examine it with the care and skill which he gained from many years of scientific work. Non-

Catholics—some of them, one might perhaps even say many of them—seem to hold the irritating and absurd idea that Catholic men of science are afraid of the hypotheses of their non-Catholic brethren. This ridiculous idea is coupled with another equally absurd, equally untrue, one may add equally insulting, which is, that the Catholic man of science, if he rejects or hesitates to accept any of these hypotheses, does so, not on scientific grounds, but because he is afraid to accept it on account of its supposed antagonism to religious dogma. Dr. Dwight shows the absurdity of these ideas in every line of his work, and, if there were no other reason to welcome it, it would be welcome because it disposes once and for all, as far as rational men are concerned, of this absurd fable.

This may, perhaps, be a not inopportune moment to say something on this method of poisoning the wells. There is something not a little irritating in the calm assumption on the part of certain writers that anyone who ventures to differ from them on a scientific point is therefore obviously and undeniably biassed and influenced by motives other than scientific in coming to the conclusion arrived at. Let me take two examples of the kind of thing to which I am alluding. In what should have been the grave, impartial pages of the Darwin *Festschrift*, issued by the University of Cambridge on the occasion of the Centenary of Darwin's birth, Haeckel, whose record hardly entitles him to criticise a man of such position, is allowed to employ the most severe language respecting Virchow on account of certain statements made by that very distinguished man. Now, in the first place, who was Virchow? Well, he was not a Catholic, not even, I believe, a Christian, but he certainly was one of the most distinguished of the biologists and anthropologists of the second half of the last century. His *Cellular Pathology* caused a revolution in medical ideas; the *Archiv* founded

by him was for many years, and still remains, one of the most important scientific journals in the world, and in a number of other ways Virchow won a fame such as but few win. It was a fame, too, which promises to be enduring, and one which is never likely to be tarnished by the kind of accusations which have been freely made respecting his critic. But Virchow had an independent mind, and was not prepared to utter the shibboleth of the day unless at the dictates of his own reason. And because he was not prepared *jurare in verba Haeckelii,* he becomes the target for the remarks of that luminary. It was not dangerous for Haeckel to enter the lists against Virchow when he did, for Virchow was then dead. The courageous Haeckel entered the lion's den "knowing the lion was not there, but dead." What is remarkable is that such statements as those I am now about to quote should have been allowed to appear where they did appear.

Virchow, according to his critic, lacked "a broad equipment in comparative anatomy and ontogeny," in other words, he did not subscribe to Haeckel's views on those subjects, nor admit the correctness of his diagrams and the infallibility of his ideas as to the pedigree of man. "In earlier years, and especially during his splendid period of activity at Würzburg (1848–1856), he had been a consistent free-thinker, and had in a number of able articles (collected in his *Gesammelte Abhandlungen*) upheld the unity of human nature, the inseparability of body and spirit. In later years at Berlin, where he was more occupied with political work and sociology (especially after 1866), he abandoned the positive monistic position for one of agnosticism and scepticism, and made concessions to the dualistic dogma of a spiritual world apart from the material frame." On this passage it may be observed how Haeckel calmly assumes that his monistic theories are the one true faith, and that a

departure from such views is a plunge into "agnosticism and *scepticism*," the latter word being a peculiarly choice example of his adoring attitude towards his own theories.

But let us proceed with our subject. In 1877 Haeckel tells us that he came into sharp conflict with Virchow. Haeckel had given an address in which he sought to prove that man, including his mental qualities, had been derived from an extinct primate ancestor. Virchow replied to this by an address on "The Freedom of Science in the Modern State." In this "he spoke of the theory of evolution as an unproved hypothesis, and declared that it ought not to be taught in the schools, because it was dangerous to the State. 'We must not,' he said, 'teach that man has descended from the ape or any other animal.'" Let it be observed that at this very time Virchow was President of the German Anthropological Society, that is, that he held the highest position in connection with the study of man which his scientific compeers had it in their power to confer on him. "Numbers of journals and treatises repeated his dogmatic statement: 'It is quite certain that man has descended neither from the ape nor from any other animal.' In this he persisted till his death in 1902." Now what is the conclusion of the whole of this matter? Obviously that Virchow was wrong and Haeckel was right. And why? Because Haeckel says so. A conclusion very unlikely to be accepted as convincing by any person who is in any way familiar with the work and merits and estimation amongst their scientific brethren of the two persons. But the point to which I particularly wish to call attention is that the attempt to belittle the opinion of Virchow on these matters is made *not* on scientific grounds, but on the excuse that his mind had become tainted by the "dualistic dogma of a spiritual world apart from the material frame," in other words, that long thought had

led him to the conclusion that there was more in heaven and earth than finds a place in the Haeckelian philosophy. Virchow, however, was not, as I have said, a Catholic, not even a believer, so far as I understand his opinions, in Christianity. How much more infamous is it when a Christian, and still more a Catholic, ventures to criticize current dogmata of science even from a purely scientific standpoint. " For if in the green wood they do these things, in the dry what shall be done ? "

So when Father Wasmann steps out to offer his opinions on organic evolution and other kindred subjects, approaching them as he does from a purely scientific standpoint, one has no reason to wonder at the reception which he meets. Father Wasmann is undoubtedly the leading authority in the world on ants and termites and their inquilines ; no one doubts that. His observations and his writings are classics in that branch of knowledge. No one, therefore, can speak of him as an amateur ; he is a master in science. Well then, how is he to be attacked ? The method is quite simple. Admit his contributions to positive science, but say that when he comes to theory he allows his scientific instinct to be clouded by his religious opinions. There are, says one of his critics, two personalities in Father Wasmann, as shown by the letters after his name " S.J." Wasmann the scientific man is excellent, but he allows himself to be influenced and his judgement to be warped by Wasmann the Jesuit. What a splendid argument, and how completely it overthrows poor Father Wasmann ! But let us turn this critic's guns upon himself. After his name are the letters " Ph.D." Is it wholly impossible that the Critic and Philosopher may not be at times warped by the Critic and Darwinian ? Is there no such thing as " Darwinian bias " or " Darwinian dogmatism " ? Professor Driesch says that there is, and contrasts the dogmatism of his followers with the open-mindedness of their

master. But enough of this. What has just been said has been said with the object of warning readers that the common method of meeting statements such as those to which I have been alluding is to follow the ancient legal advice: "No case; abuse the plaintiff's attorney!" Fire the accusation that he is "reactionary" against your opponent, and go your way satisfied that you have destroyed him and his opinions for ever.

Reactionary motives! That is what will be said about Dwight and his book, perhaps is being said, as it is said and has been said and probably will continue to be said about any attempt to criticize the popular scientific idol of the day. Yes: but by whom will it be said? Not by men of science, for no honest man of science can read Dwight's book and think of it as other than candid and fair; not even by the ordinary uninstructed but impartial reader, for to him again the candour of the writer must and will be obvious. No; it is the Haeckels and the jackals of the Haeckels of this world who, beaten on their own field and totally unable to answer the arguments advanced, choose this method, the old Ephesian method of crying "Great is Diana of the Ephesians."

A fair hearing—that is what may be asked for Dr. Dwight's book; and surely this brief account of his life shows that its author is entitled to that indulgence.

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